

Interactive comment on “Discussing the role of tropical and subtropical moisture sources in extreme precipitation events in the Mediterranean region from a climate change perspective” by S. O. Krichak et al.

Anonymous Referee #1

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The scientific community has recently paid much attention on the study of extreme precipitation events (EPE), particularly in the Mediterranean Region (MR). Therefore a review paper which discusses the major research effort on this subject as well as the state of the art on the investigation of mechanisms related to the role of tropical and subtropical moisture sources on the occurrence of EPE in Europe and the Mediterranean region would be most relevant to be published on NHESS. However as it stands this manuscript requires serious major revision before being accepted for publication. A review paper should be, in my opinion, a text which thoroughly presents and sys-

C1411

tematizes the most important knowledge on a certain theme, to the present date – in this case MR EPE. As the authors state in the abstract, “The aim of this paper is to present a discussion of the major research efforts on this subject and to formulate a summary of our understanding of this phenomenon, along with its recent past trends from a climate change perspective”. In my opinion, in order to be a review paper, this manuscript should present, describe and discuss more deeply the several papers and issues related to MR EPE.

To justify the relevance of the study, the authors present several historical MR EPE examples and suggest the link between those events and ARs. However, this is performed in a very superficially way (e.g. pg. 3985-86), with expressions like “it appears evident”, “It may be speculated that”. The authors should avoid expressions like “Apparently” and “It appears”. While the issue of tropical and subtropical moisture sources/transport is still debated in the scientific community, we would expect that a review paper would discuss more comprehensively these examples, presenting evidence from published literature and/or adding new perspectives to known events. The examples chosen should be fully presented in order to enlighten the readers on the mechanisms involved or critically discuss the several perspectives still in debate in the community. When mentioning a “simplified conceptual model” (Pg. 3987) the authors should present the model and discuss its importance. On a literature review paper, examples should not just be referred – the reader is expecting to see some systematization of the examples, with a presentation of an overview on the new insights resulting from the examples. Additionally, some of the most relevant figures from the literature should be presented to highlight the most important results. Alternatively, some new schemes might be developed and presented. This would be an important contribution to the scientific community and to young researchers who may use a review paper to further contribute to our knowledge on the subject.

Finally, the authors should develop further the review on MR EPE and its connection to climate change. Presently, this section is mainly based on the paper by Krichak

C1412

et al. (2014b) and results for a very specific area in the MR – the southern Levant. A review discussion should mainly focus on the MR as a whole, eventually discussing the different characteristics and mechanisms on distinct MR sub-areas. Other published research should be included in this review. The discussion on the link between Arctic sea ice and NAM/AO/NAO is mostly too general; there should be some more detailed explanation on how these affect the MR.

Specific comments

Pg 3985, ln 21-22 – The sentence seems incomplete and out of place

Pg 3985-86 and discussion of Fig. 1 – The AR is not evident and fully characterized only from the PW pattern shown in the figure – some further discussion should be presented on the criteria for attribution of an AR (strong winds, length and time criteria).

Pg 3989, ln 6-10 – The sentence is not clear. What do the authors mean by the expression “without triggering a storm earlier”? In this paragraph the authors might further develop the controversial aspects and open issues on this theme which still deserve future research.

Pg 3991-92 and discussion of Fig. 2 – The discussion of these new results does not seem adequate nor representative in this review paper. How can these results be representative of all EPE in the all MR? Can these results be generalized for the all MR? Are there other studies which confirm this conclusion? The sentence “It is likely that this relationship for the southern Levant holds for much of the MR.” (pg 3991, ln 22-23) is speculative and should be confirmed. The results presented on the following paragraph are, again, only for the southern Levant region. In my opinion, they are not relevant in this paper. I consider that either the authors keep the paper as a review, or if they want to present new results they should make it more detailed, describing data and methodology.

As a final point, the role of global warming in recent past trends in extreme weather

C1413

events over the Mediterranean region should be better discussed.

Figures – the manuscript has few figures, with poor technical quality. The review would greatly benefit from some extra figures and/or schemes further documenting the AR contribution to MR EPE.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 3983, 2015.

C1414