

Interactive comment on “Intra-annual variability of the Western Mediterranean Oscillation (WeMO) and occurrence of extreme torrential rainfall in Catalonia (NE Iberia)” by Joan Albert Lopez-Bustins et al.

Anonymous Referee #2

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General Overview:

The authors analyzed the intra-annual variability of the Western Mediterranean Oscillation and occurrence of extreme torrential rainfall in Catalonia (NE Iberia). Despite the target region and topic is of interest to be study due the possible socio-economic impacts of the torrential rainfall, the manuscript in the present form do not add much to the present knowledge. In addition, it has some very important methodological and organizational issues which are listed below:

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- 1) My main concern is that the manuscript fails to add new knowledge to the literature. In the present form, the manuscript is rather descriptive specially in section 4.2 and 4.3 where there is a statistical description between WeMO and the torrential rain which was previously known. From my point of view, there is the lack of understanding what is the physical mechanism which are behind the extreme torrential rainfall in Catalonia, for example, the atmospheric forcing, the role of SST, or even the soil moisture availability.
- 2) Figure 2a) is computed with data from where? The monthly series provided by the Meteorological Service of Catalonia?
- 3) The authors use a fix threshold to define the extreme torrential episodes which is >200mm in 24h. L168-173. I do not agree with this sentence. Based on my experience I can imagine that precipitation >100mm in a relative larger area will have more impacts than a precipitation >200mm only recorded in one single weather station. Therefore, I encourage the authors to think of a way to define the torrential episodes based not only on the amount of precipitation but also on it's spatial extent.
- 4) There is an inconsistent between the period of analyses. On line L126 is mentioned 1950-2015 and on L167 1950-2016.
- 5) The authors need to include a better description of the weather stations. How many of them are at a daily scale vs semi-hourly data. Since which year do you have access to automatic weather stations?
- 6) L220-222 The WeMo is computed using SLP from the weather stations mentioned in the text? They are quality controlled?
- 7) In Figure 3 and Figure 8 the authors used the outdated NCEP/NCAR reanalysis. Please use ERA5 instead.
- 8) Figure 7 d , e ,f). These results are not mentioned in the text. I would exclude it from the manuscript.
- 9) L268 The mean and standard deviation is computed at an annual scale or at a day

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level?

10) Figure 4. Why this division?

11) Regarding section 3.3, why don't the authors use a moving average instead of artificial 10-day or 15-days intervals?

12) L468-470. I don't think that 4 weather stations are representative of southern France. I would delete everything related with these 4 weather stations from the text, including Figure 9.

13) L527-529 I agree with the authors and I think an analysis on this, among physical mechanisms (see comment 1), should be included in the new version of the manuscript.

Therefore, I recommend the major revision of the manuscript.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2019-374>, 2019.

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