Interactive comment on "Intense precipitation events in the Central range of the Iberian Peninsula"

by Manuel Mora García et al.

Response to RC2

Main comments:

1. There are issues with regards to the methodology.

a. More details need to be provided on the kriging method, as it will have an impact on the results. Please elaborate which technique and why it was chosen.
Precipitation is very difficult to interpolate in mountainous areas, so that the impact of the interpolation method on the area-averaged precipitation is small and, in consequence we used the kriging interpolation method because is the de default method in the ArcGIS© software.
For more information,
[https://desktop.arcgis.com/es/arcmap/latest/extensions/geostatistical-analyst/understanding-ordinary-kriging.htm]

b. More details are needed on how the precipitation episodes were defined. From Table 1 the length of the events varies significantly (from less than one day to eight days), which makes it very hard to compare the precipitation totals in Table 2. In the abstract you mention 24 hours, but I cannot find anywhere that this is mentioned again in the text. On page 4, it is mentioned that the episodes were chosen with 'the heaviest precipitation', but over what time period, over what area/which rain gauges? For Case study b, you acknowledge that the total precipitation was less due to the short duration of flow perpendicular to the mountain range, yet, is this 30-40mm in 18 hours similar to other events, less than other events? Calculating the maximum hourly precipitation, or daily precipitation amount could be a solution.

An episode was selected if the precipitation accumulated in 24 hours had values > 100 mm in at least one observatory inside the selected area. Table 2 shows the values of accumulated precipitation in the observatory that reported the maximum value. Following your recommendation, we calculated the maximum daily (new column in table 2). The case study b is not an episode of intense precipitation due to its short duration. This was precisely what we wanted to show. The data of this episode were taken into account in the averages by mistake. This has been corrected in the revised manuscript.

c. The choice of models. By referring to the ECWMF model, implies the forecast product – but there is no reference to confirm, and at which time steps the variables were selected. And are the two datasets really comparable? It would be preferable to either use one dataset over the entire period, or use ERA40 + ERA Interim where there is an overlap and you can assess the differences between the two datasets (1979 –2002).
Similar Reply to RC1
". . .For the period 1958-1978 the JRA55 reanalysis should be used, which are available since at least 2014, after that ERA-Interim data are available since 2011 at

least. These data sets are much more homogeneous than ERA-40+ operational ECMWF data. . .”

We think that the JRA55 reanalysis had an objective to improve information, mainly in the area of Asia. In addition, we do not believe that there is a clear pronouncement of the scientific community on which of the models of reanalysis is the most adequate, or if the JRA55 is better than ERA-INTERIM.

There are numerous studies comparing reanalysis models, but all agree on the great similarity of results. In any case, they present some differences in certain parameters, mainly those that depend strongly on the altitude, seasonality or area of study.

For example, in this reference https://climatedataguide.ucar.edu/climate-data/jra-55 there are some weaknesses presented by JRA55:

Key limitations:
As with most reanalyses, diagnostic variables including precipitation and evaporation should be used with extreme caution.
Dry bias in upper and middle troposphere and in regions of deep convection
Time-varying warm bias in the upper troposphere

Accordingly, the calculation of the moisture flows is also not very reliable in the JRA55. To corroborate that there is no unanimity in the model to be chosen, we indicate an analysis of the data from Ireland, in which it is not clear which of the models, ERA-Interim, ERA-40 or NCEP, is the most appropriate. http://eprintsprod.nuim.ie/2513/1/MooneyMulliganFealy2011.pdf

Moreover, the meteorological fields used in this study (wind and moisture) are in the pressure levels of 850 and 500 hPa, that is, above the PBL. This means that the horizontal resolution has not a great impact.

d. Not clear why the one particular point for the analysis was chosen. Because the windward slope is steepest? Would the values in Table 1 change much if point what somewhere else?

The particular point was chosen because in that point: i) the Gredos range reaches its maximum altitude and, consequently the southerly wind must surpass a big difference in height (1500m), and ii) the slope is maximum. The values in table 1 will be not very different is other points were chosen, because the meteorological fields are outside the PBL. Data of precipitation will be different in other point, but we wanted to show the values where the orographic effect is expected to be greater.

2. Suggest some reorganizing of the theoretical concepts, study area and data. Only some of the indices are introduced (e.g.., Froude number, although somewhat confusingly the equation is left out until the study area section; the index by Lin et al 2001 is introduced, but not mentioned in the list of indices in the next section, but then is included in the results discussion). It is interesting to compare these indices, so I suggest all the indices that you use are included in the theoretical concepts (at least a brief explanation about what is moderate/high values or a

reference to where one can find out), including why they were selected as not all indices are included, such as deep layer shear. To provide an example, you do not consider any wind only measurement for each episode, yet you discuss wind intensity as being an indicator for convective vs stratiform precipitation (page 6). Furthermore, there is inconsistency on the flow regime types: On page 3, Type I-III are all for convective systems (with differences in propagation), yet on page 4 you refer to type II as stratiform.

We agree and we have reorganized the manuscript.

3. There are inconsistencies between the conclusion and the results. For example on page 8 lines 1– 2 state that . . .."the moisture flux associate with the cases of heavy orographic precipitation considered here was. . .., and both the dry and moist Froude numbers were >1". Although clearly from Table 1, some of the Froude numbers are less than one. In the abstract, you state "all events were associated with a south-westerly flow, a low level jet. . ." yet you only consider the composite of 19 events and not the individual events. Did you assess each of the events individually? Even if plots of the individual events are not shown, it would be useful to know that each event was indeed associated with the above synoptic situation.

Lines 1-2 on page 8 have been changed. The characteristics of each individual event are similar to those of the average (this is indicated in the revised manuscript). It should be noted that the inclusion of synoptic maps for individual events would make the paper excessively large.

4. The discussion on page 6 is confusing. Where do the values 1.6km and 35km come from and why are they acceptable? 5km is the resolution of what? The DEM you used in ArcGIS? Similarly, doesn't the Type III imply the presence of a convective system propagating similar to the flow, not that it is just a convective system?

The mean slope is 0.05 (which is now indicated in section 2 page 4, line 17) ≈ 16/35. The value 0.09 is the maximum slope. This is corrected in the revised manuscript. Line 7, page 6 is also corrected

5. The language also needs to be improved. Some examples:

a. Page1 lines 18-19, what increases? Do you mean ". . ..the forecasting of precipitation is therefore difficult, particularly for forecasts with coarser spatial and temporal resolution"

The text has been corrected.

b. Page 4 lines 4 – 5: "to increase to higher areas" what do you mean?

The text has been reordered and changed

c. In the abstract: "from 19 episodes, with the highest average values for the study area, of precipitation accumulated within 24 h, occurring between years 1958-2010" Do you mean "from 19 episodes, which have the highest average 24 hour precipitation amounts in the study area between 1958 and 2010"

The text has been corrected.

6. The figures and tables also require some work (see comment from the previous reviewer). Some additional/specific comments:

In accordance with suggestions from the Reviewers, some figures have been improved.

a. Table 1: Caption needs improvement. It states "the values at 850hPa", but CAPE is measured only at the surface. TT also uses information at other pressure levels. The caption should also indicate that these are the values at the point indicated in Figure 1.

The caption of Table 1 has been corrected.

b. Table 2: where is case 19? And what is the time period (for the entire days in Table1)? It would be useful to know the number of hours considered for each episode.

Case 19 has been added to Table 1 and Table 2, but is not taken into account to calculate the average value, since it was a singular case with little precipitation.
Unfortunately it is impossible to know the number of hours considered for each episode and we can only assign a minimum period of 24 hours, as shown in Table 2.

c. Figure 3 – how was the precipitation calculated (blue line)? Is this average precipitation for the episodes? No horizontal distance on the x-axis (difficult to estimate using figure 1b). Can you use something similar as in 7b?

Figure 3 represents a cross-section of average precipitation of all events (line) and an orographic profile along the line AB, described in Figure 1b.

d. Figure 6 – difficult to determine the location, perhaps include lat/lons. It would be useful to include the horizontal line that use in 7b as well for reference.

Unfortunately the quality of this figure can not be improved. According to the comment we have added the latitude and longitude to the figure, for a better understanding.

e. Figure 7 Where is this profile for? (see comment above). And what is meant by 'precipitation profile'? Average for the episodes, or is it just one episode?

Figure 7 refers to the case study from 23 to 25 November 2006, and it shows a cross-section of the altitude and precipitation vs horizontal distance.