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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "PREGRIDBAL 1.0: towards a high resolution rainfall atlas for the Balearic Islands (1950–2009)" *by* Toni López Mayol et al.

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General Comments

The article deals with the analysis of daily precipitation from AEMET in the Balearic Islands for the period 1950-2009. The authors have compiled an extensive dataset on precipitation observed from the ground in this archipelago and analysed their trends.

The manuscript has a number of merits: It treats a subject of relevance to the audience of NHESS. It is based on the complete dataset of ground precipitation stations of AEMET in the Balearic Islands. Finally, the analyses produce interesting results, as is pointed out in the conclusiond.

However, there are some questions that the authors should solve in a revised version

of the paper before its publication. The main problems are focused on: a) Give proper credit to recent bibliography on the matter b) A poor quality control c) Impact of the orography in the rainfall distribution d) Completeness of lack of data e) Only trends with a statistical significance above 95% or 90% are usually accepted in the trend analysis of precipitation (mainly in these studies potentially related with climate change). Then, you cannot generalise the precipitation decrease and cannot accept as representative those trends with significance inferior to 90%.

The acknowledgements suggest that this PREGRIDBAL database has been obtained by public funding. Consequently, it should be open access (at less from 1950 to 2009). Could you include in the paper the link to this database? It would increase considerably the impact and diffusion of this paper and the authors could require to the users of this database to refer to this paper.

If the dataset used or created by the authors is not made public the scientific contribution of the paper is limited by the fact that other researchers will not be able to verify or question their conclusions, improve on their methods or make further analyses.

The English language should be revised.

In conclusion, in my opinion, this paper deserves publication in NHESS after a few improvements have been made. Please, take into account my previous comments and the following ones.

Abstract:

Include the number of stations you have used in the paper for the trend analysis. If you focus the paper on the period 1950 to 2009, why you refer to the previous years? Eliminate these conclusions that are non significant statistically. You speak about weather patterns but they are not explained in the text.

Introduction:

You cite the 4th Assessment Report of the International Panel on Climate Change

(2007). This work has been updated in 2014! Please, refer to the AR5 of IPCC, 2013 (WGI) or 2014 (WGII, WGIII and synthesis).

The Introduction does not give proper credit to the most recent literature on precipitation trends in this region, Spain or the Mediterranean Region, neither to the other daily/monthly databases created in Spain. Consequently, the authors do not consider previous results, and do not include a comparative analysis with them. They speak about the Spain02 daily precipitation database built from 1950 up to 2003 (Herrera et al., 2012), and the trend obtained by Homar et al (2010) for the Balearic Islands. But they do not compare with the trends obtained from this database by other authors for the entire Spain or specific regions near the Balearic Islands. It would be also interesting to present the trends obtained from the MOPREDAS monthly precipitation database (González-Hidalgo et al. 2009 and 2011). These databases, built from AEMET data, have been widely used to explore regional patterns and trends (Vicente-Serrano et al. 2010; Turco and Llasat, 2011). In spite of the different time and/or spatial resolution, it would be necessary to improve the paper and compare with the trends obtained from these databases (or another Mediterranean ones), especially if your results refer to annual values.

You introduce the 1-km resolution grid from Guijarro (1986) over the Balearic Islands for the period 1961-1980, telling that this author have applied the multiparametric interpolation (including height, sea distance, terrain gradient,. . .). However, it is not clear if you apply this interpolation to this paper

Data and methodology:

Please, include the main physiographic features of the Balearic Islands.

To create a precipitation map of the entire archipelago with only 4 stations can introduce some misunderstandings. Please, indicate from which year you would have a minimum number of stations that could be enough representatives, and start the regional analysis on this year. To which period refers the sentence “Average spatial den-

sity of stations on the grid, leaving aside asynchrony in the running of many of them, is 0,08 stations per km2 (or 3,5 km average distance between neighbouring stations)”?

I do not understand the objective of the sentence “thus solving the estimation uncertainty (Dingman et al. 1988): an improved technique after Krige’s work (1951) and which has recently advanced drastically in its application in the climatology field and its respective variables (Moral, 2009)”. The references of Dingman and Krige are very old, and the krigging application is usual to analyse precipitation fields.

Some aspects of the methodology deserve clearer and more elaborate explanation: âĂć Which is the criterion used to consider a precipitation day? More than 1 mm/24h? âĂć The quality control is poor. To eliminate non-physical precipitation values is not enough. Have you made any geographical comparison or correlation between daily values? Perhaps AEMET has applied a preliminary quality control. Do you know it? âĂć Have you applied any homogeneity criteria to each series? âĂć How have you complete the gaps? You could fill the gaps in precipitation records through bilinear regression or any other methodology. âĂć How have you considered the influence of several physiographic characteristics on precipitation? âĂć Why you define the anomaly as the rate between daily rainfall with respect to average annual precipitation, and not to the monthly one?

You say that you have applied ordinary Kriging for the interpolation, using the daily anomaly with respect to the annual mean for all available observations each day, but this methodology don’t work when strong changes in the orography are produced. This is the case of the Tramuntana mountain range. You should consider the physiographic influence of elevation and slopes orientation. One first approach would be the application of the precipitation lapse rate (PLR) that is represented as the ratio between changes in mean annual precipitation and changes in elevation. There is some literature about the influence of the topography and the PLR values like Johansson and Chen (2003), Durán et al (2013), Marquínez et al (2003) or Naoum and Tsanis (2004). Another possibility would be the application of the methodology developed by Guijarro

(1986)

Particularly, in the case of Spain, regional linear regression of mean annual precipitation and altitude was assumed in MAGRAMA (2004). In this work, virtual precipitation series at higher altitudes were estimated to improve the interpolation of monthly records by means of inverse distance weighting (IDW) algorithm and an average PLR value was obtained in basis to precipitation series provided by AEMET. Attending to the fact that this work used all the AEMET precipitation series, the Balearic series were also included.

Although your work refers to daily data, some considerations following my previous comments should be considered in the interpolation when you built the PREGRIDBAL database for the Serra de Tramuntana Region.

Paragraph 2.3.1 should be eliminated. This kind of information should be included at the beginning of the section 2, when the AEMET network and the number of stations used should be presented.

The tendency methodology applied is not the usual nowadays and it should be necessary to solve previous questions, like if the null hypothesis in the MannKendall test is satisïňĄed (on the contrary there is more probability of detecting trends when actually none exist). Giving the importance of the results, other methodologies like the “moving block bootstrap” method developed by Kiktev et al. (2003) or other explained in Moberg and Jones (2005) should be applied.

The pattern trend should be analyzed with caution, considering only the maps with ïňĄeld signiïňĄcance at least greater than 95%, (i.e. Livezey and Chen, 1983)

Products:

Change the name of this section by Results.

Section 3.2 should be moved to Data and Methodology

All the trends with a significance <90% are not representative and should not be discussed. Please, re-write this section.

Please, avoid specific names of cities that are not showed and marked in the maps.

Attending that you have daily data it would be interesting to analyse the ETCCDI index evolution.

How do you relate the analysis of the two cases of study with the rest of the paper?

Please, if you introduce Figure 5, it should be necessary to introduce any short explanation or reference to justify why this year recorded the maximum annual precipitation

Please, modify some formal aspects of Table 1

Why do you split the period in two: 1950-1979 and 1980-2009? Why do you not use a running window?

Conclusion:

Please, focus only in the results with statistical significance>90%.

What do you mean with “ This positive tendency could be attributed to the effect of friction which the city has on atmospheric currents or also to the pollution generated in the urban area”?. Are you mixing turbulence questions with precipitation patterns? How do you can justify it? The pollution level and size of Palma de Mallorca could justify it? In the abstract you speak about changes in the precipitation patterns, but you do not explain anything about it. Which are these changes in precipitation patterns? Have you analysed changes in the frequency of the weather maps (or Principal Components) associated to precipitation?

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