

Interactive comment on “Reconstruction of rainfall in Zafra (southwest Spain) from 1750 to 1840 from documentary sources” by M. I. Fernández-Fernández et al.

Anonymous Referee #3

Received and published: 5 January 2012

Review of the paper: Reconstruction of rainfall in Zafra (southwest Spain) from 1750 to 1840 from documentary sources by M. I. Fernández-Fernández et al.

Of all the meteorological variables, precipitation is characterised by the greatest spatial and temporal variability. Therefore, reconstruction of this element based on documentary evidence is a very difficult task, which should be done carefully. The paper reviewed here deals with such a reconstruction for southwest Spain. For this purpose, the authors of the paper used weekly reports containing weather notes for the period 1750–1840, written by accountants working for the Duke of Medinacelli. Generally, more information about precipitation changes in historical times for different parts of the

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world, including Spain, is urgently needed. Reconstructed data must be, however, of good quality in order to give a reliable view of amounts and other precipitation characteristics. Being the last reviewer to write comments on the manuscript, I must underline that I completely agree with all the comments provided by the first two reviewers, and therefore there is no sense in repeating them again here. The main weakness of the paper lies in the methodology used and the statistical processing of the data. Besides the propositions of the first two reviewers, which in my opinion should significantly help to improve the paper, I also see other possibilities. From Fig. 6 it is clear that there is probably quite a good correspondence between the precipitation in Zafra and the series from Gibraltar, Cadiz and Lisbon. (By the way, the authors should calculate the correlations between the series there). If so, there is an overlapping period (1815(16)-1838) of instrumental precipitation in Cadiz and Gibraltar on the one hand, and the series of indices for Zafra on the other hand. These series should be used for calibration of the data, instead of the data from the contemporary period, 1960-1990. Still, there is a lack of series of instrumental data for the verification of obtained algorithms. To overcome this problem I suggest using daily data from the contemporary period to check if the proposed three indices allow for correct reconstruction of monthly precipitation sums. If so, the algorithm established based on the data from the overlapping period should work correctly, and should allow for a reliable reconstruction of the precipitation for historical times. In my opinion, the section 'Other weather phenomena' should be omitted because the results presented, e.g. in Fig.9, evidently show that the data are wrong, and therefore should not be published uncorrected. In addition, the elaboration of frost and storms is beyond the scope of the main subject of the manuscript. In conclusion, similarly to the two previous reviewers, I estimate that the paper contains significantly more weaknesses than advantages and therefore should not be published in CP.

Interactive comment on Clim. Past Discuss., 7, 3895, 2011.

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