

The manuscript is an interesting work on historical droughts in Northeast Iberian Peninsula, but in my opinion it needs some revisions before publishing:

- 1) In the literature you can find different definitions of the drought concept (atmospheric, meteorological, hydrological, agricultural), depending on the physical variable studied (relative humidity, rainfall, other elements of the hydrological cycle), and the duration of the event (days, months, seasons). I understand that here the authors are studying agricultural droughts, due to the origin of the data (rogations linked to agricultural production). In any case, it would be important to precise this point.
- 2) The nature of rogation ceremonies must be explained with more detail. For instance, is it possible to find a 'preventive rogation', that is, a ceremony organized before the event occur? In this sense, the date of the rogation is an important information. It may be the case that a dry winter provoked the rogation, but timely spring rainfalls yielded a good harvest. In that case, can we speak on 'drought'? In relation to previous comment, perhaps here we could speak on dry winter (meteorological drought), but not on 'agricultural drought', and, in consequence, this event is not comparable with other characterized by the water deficit during an entire year. The 'annual' index (from December to August) may mask important intra-annual fluctuations, in my opinion it is preferable to divide the information into seasonal indices, following the different phases of the plant growing, from seed (autumn) to harvest (summer). In addition, all rogations are linked to cereal production? Other plants (fruits, olive trees) have different climatic limitations, and it would be possible that a single meteorological event (for instance, dry spring) was harmful for a specific plant, but not for another (for instance, the barley is more tolerant to drought than wheat).
- 3) I have doubts on the classification of the rogations (lines 205-207, Table 2). Were the ceremonies the same in all the cities and during the whole time period, from 1650 to 1899? Severity indices are based on the type of ceremony, but is it a reliable criteria? In the discussion (lines 404-411), authors say that 'an index of level 2 does not necessarily imply that a drought was twice as intense as a drought classified as level 1, nor that the change in the intensity of droughts from level 1 to level 2 or from level 2 to level 3 has to be necessarily equivalent'. In that case, how must we interpret these indices? In my opinion, these indices only specify the nature of the ceremonies organized as response to natural hazards, but do not inform on the severity of the climatic event. In consequence, what is their utility from a climatic point of view? In my opinion, the binomial distribution (occurrence or not) is the more appropriate statistical approach to the treatment of this information.
- 4) Clustering is an appropriate tool to classify and group local series into regional series. There are very different clustering algorithms, hierarchical and not hierarchical. Why have you used Ward method with Euclidean distance, and not, for example, the non-hierarchical k-means, or other methods as the principal component analysis? Results of clustering must yield groups more or less

homogeneous, but the chosen number of clusters is normally arbitrary. Why do you distinguish between Mediterranean and Ebro Valley group (dendrogram, Figure 3), if, as you say (lines 450-451) ‘the high correlation between DIEV and DIMED is suggesting similar climatic characteristics’?

- 5) Validation of the regional drought indices is made using the overlapping period 1786-1899 between documentary and instrumental data. But, as you say in the discussion (lines 390-392) ‘the apparent low frequency of rogations in the 19th century could be explained by a combination of political instability, and the loss of religiosity and historical documents’. I would add changes in the socioeconomic structures, organization of the cereal production, agricultural techniques, etc. In consequence, this period is not valid to calibrate and/or validate the rogation series in previous centuries. The cultural background, economic organization and technology of the 19th century was not the same that in previous centuries, and calibrations established for 19th century are not applicable to 17th century! In fact, you do not use this calibration (regression in Figure 5) to interpret previous data, only to validate the index during the 19th century. Besides, this analysis is only made for DIMED (Barcelona), and not for the other points in the studied area. I suggest to remove this analysis (and the Figure 5).
- 6) Superposed epoch analysis (SEA). Although you give a reference, a brief explanation on the basis and procedures of this method would be important.
- 7) Minor questions:

Line 301: ‘The cluster analysis (CA, see methods) using the DI of the 13 locations for the period of 1650-1899 AD revealed three significantly coherent areas...’ Erratum, I suppose, clustering is made using the period 1650-1770, common to all the stations, although the classification obtained is after applied to the complete period until 1899.

Lines 373-375: ‘However, two years after the Tambora eruption in April 1815, there was a significant ($p < 0.05$) increase in the three drought indices...’ However? The time life of volcanic aerosols in the atmosphere is around one to two years. The Tambora aerosols caused a radiative forcing of the global climate system of about 5.6 W/m^2 for one to two years following the eruption (Brönnimann and Krämer, 2016). In consequence, this increase in the drought indices may be caused not by the volcanic eruption, but by the return to ‘normal’ conditions (or not forced climate variability).

Lines 417-418: ‘the local series are compared with the regional reference series as a basic element of quality control’. But, if the regional series is obtained from the average of local series, here we have a circularity problem.

Line 432: ‘the local series are separated by tens or hundreds of kilometers’. If you speak on meteorological droughts, this is not a problem, because the dynamical

conditions provoking dry conditions are associated to the predominance of anticyclonic conditions, and the spatial extension of an anticyclone may be much greater. Again, we are speaking on the drought concept. Meteorological, hydrological, agricultural?

Figure 5. Significance level must be added in the figures. D), E), F), correlation is the Pearson correlation coefficient?

Figure 7. The legend is arbitrary, why do you distinguish between 2.1-2.6 and 2.7-3 DI values?

Reference

Brönnimann S, Krämer D. 2016. Tambora and the 'Year Without a Summer' of 1816. A perspective on Earth and Human Systems Science. *Geographica Bernensia* G90, 48 pp, doi:10.4480/GB2016.690.01