

General comments: In this paper, meteorological droughts affecting the Miño-Limia-Sil Hydrographic Demarcation during the period of 1980–2017 are identified and assessed using the one month SPEI index (and, in some cases, some other temporal scales). In this way, the problems associated with droughts, their origin and their impacts are analyzed at a regional level. It is shown that the driest/wettest conditions occur under some particular Circulation Weather Types. In addition, some teleconnection patterns seem to favor more/less frequent dry conditions with different temporal scales. Finally, soil moisture and river stream flows are also related to drier or wetter conditions in previous months. My general impression is that many of the presented results are not new, since most of the results shown in this document are in agreement with those presented in previous papers on droughts in the northwest of the Iberian Peninsula. Probably, the most original aspect of this paper is that it shows that the methods previously used in the NW Iberian Peninsula can be used successfully in a much smaller region, such as MLSHD. Anyhow, I think that this paper is acceptable for publication with some revisions and clarification.

Specific comments: In general, the indices used in the paper are described very diffusely and only some random information about them is provided to the reader (of course, references are provided on how those indices are defined. But further explanations would help to better understand what is presented in the paper). I think more information has to be provided on how those indices are calculated and what they represent. For example, why doesn't the 'materials and methods' section include a brief definition of SPEI and SSI and the way they are computed? Equations 1 and 2 define E_{t0} (E1) and $Rn_2(S)$, but the text does not include the definition of some of the indices most frequently used in the paper (SPEI, SSI, severity...) or, for example, the way in which the statistical significance of the wavelet coherence is calculated. Some information about the data is missing as well. Are the Miño and Limia rivers discharge series affected by reservoirs or by any other human regulation activity? This information should be included in the description of the data and considered in the discussion of the results. In the conclusions section, it is explained that the classification of the CWTs is daily. I think this information should be included in the description of the data.

We appreciate your advices to improve the manuscript. The methodology section was improved in order to provide a better description of the indices utilized, the calculation of drought indicators, the computation of WTs and the Wavelet coherence significance.

The Atlantic Multidecadal Oscillation is known to be characterized by a period that varies from approximately 40 to 80 years (even if a band of 20-30 years is accepted, the period studied in this paper does not even include two complete cycles of such C2 NHESD Interactive comment Printer-friendly version Discussion paper an AMO oscillation). Therefore, it is very difficult to assess the impact of AMO with a study period of less than 40 years. If the maximum coherence period between AMO and SPEI1 is 6 years (figure 11) then it is not adequate to describe it as multidecadal coherent oscillation. My opinion is that the analysis of the impact of the AMO is too noisy (and not multidecadal enough) and that it should not be included in this paper.

We acknowledge your advice! We agree and consequently the AMO analysis and discussion was removed from the text.

Other suggestions, corrections and typos:

Abstract: Is it acceptable to define acronyms in the abstract? If so, MLSHD is defined for the first time in the abstract and it is not needed to re-define it in the Introduction (P3-L6)

The acronyms were deleted from the abstract.

Abstract-line 19: 'The results showed that atmospheric circulation from the southwest, west, and northwest were directly related to dry and wet conditions': To both dry and wet conditions?

Thank you. This sentence was modified:

"The results showed that atmospheric circulation from the south-east/west, east/west, and north-east/west were directly related to dry/wet conditions in the Miño-Limia-Sil Hydrographic Demarcation during the entire climatological year"

Abstract-line 22: 'the major teleconnection atmospheric patterns': change to "the major atmospheric teleconnection patterns"

Changed

Page 3-Line 15: Delete '(Figure 1)': the MLSHD and figure 1 are already referenced a few lines before.

Deleted

P4-L15: define the variables in ec. 1

You are right They are now defined in the manuscript

P4-L17: SPEI is said to be a multiscale index, but it is not clear what is the advantage of this multiscale character. In fact, most of the paper deals only with SPEI1.

Yes, we mostly utilized the SPEI1 to identify meteorological droughts, which we believe are the best related with different WT's. Besides the meteorological drought can be perceived as the initial cause of other types of drought. The multiscale advantage permit to assess whether this accumulated dry/wet conditions impact other steps of the hydrological cycle; in this case the soil moisture content and the runoff. The explanation was improved in the manuscript.

P4-L21: 1954-2014: 61 years, thus about six decades, not five.

Changed

P5-L13: ERA-Interim reanalysis for the period 1979-2017. If not, the period 1980-2017 cannot be set for all the analysis in this study (P6-L13).

Thank you, it was corrected. But the correct study period is 1980-2017 in order to fit a temporal scale with complete data of all variables (in this case the river discharge was complete from 1980 to 2017).

P5-L18 to L23: a better explanation of what 'pure' or hybrid' WT's are (and about the implications of WT's being 'pure' or 'hybrid') would be appreciated. A mathematical definition based on poorly defined parameters is not enough. Any mention to those 16 'hybrid' circulations could be removed from the main text since they are not mentioned in any other section of the paper.

The description of the methodology referred to the weather type computation was modified in order to provide further details. The difference between pure and hybrid types was introduced into the text as well as a description of how both types are considered in the percentage contribution.

“According to the methodology developed by Trigo and Da Camara (2000), 10 different “pure” WT’s can be identified, namely Northeastern (NE), Eastern (E), Southeastern (SE), Northwestern (NW), Western (W), Southwestern (SW), North (N), South (S), Anticyclonic (A), and Cyclonic (C). Pure directional WT’s (NE, E, SE, NW, W, SW, N, S) were those showing $|Z| < F$ with the direction defined by $\tan^{-1}(WF/SF)$ (180° added if WF is positive). If $|Z| > 2F$, then the circulation would be considered C (if $Z > 0$) or A (if $Z < 0$). As not all the circulation patterns could be associated with a pure (directional/cyclonic/anticyclonic) type, 16 hybrid circulations were defined as a combination of A and C circulation with directional WT’s. In this case, $F < |Z| < 2F$.

The methodology here described is able to daily identify the weather pattern (from the 26 listed before) presented over the area of study. From this daily information, and in order to study the WT’s influence on monthly SPEI series, the monthly frequency of occurrence for every pure WT’s is computed for the period 1979-2017. In the frequency computation, the 26 WT’s are regrouped in the 10 “pure” ones. This procedure was realized following the same approach applied in Trigo and DaCamara (2000) in which the hybrid types were included into the corresponding pure WT’s with a weight of 0.5, being the 10 final number of WT’s analysed in this study.”

P7-L12: ‘...occurred between December and February...’: ?

It was changed for “from December to February”

P8-L6: ‘...the length of these episodes increased after 2003’: Not the length, it is the frequency of long episodes what is higher. Long episodes can be found in 1988-1993 & 1980-1983, but after 2003 they are more frequent.

Thank you. It has been corrected on the text.

P8-L16-20: the trends described in these lines are far from being statistically significant (p values of 0.26, 0.52 and 0.26!!). Thus, it is difficult for me to understand why authors make so much emphasis in these trends.

The paragraph was modified according to the reviewer comment and this result was removed from the abstract as it not represents a relevant conclusion.

P9-L4: What do the numbers included as ‘severity’ in table 3 mean? How are these ‘severity’ values calculated? Same comment is applicable in table 2 and P8-L18. The definition of ‘severity’ and how it is calculated should be included in section 2.

In section 2 as added the explanation:

- The duration is computed as the sum of all months from the onset with negative values, the peak is the month in which the episodes reach the highest value of SPEI1, and the severity is calculated as the sum of all SPEI values (in absolute values) during the episode.

P11-L1: Pressure values in figure 4 are very small and difficult to read. The caption of this figure could include that reddish (blueish) isolines represent high (low) pressure values.

The figure and caption was changed to solve this!

P11-L18: Figure 5 caption: what do ‘X’s in the figure mean? I guess they represent not significant correlations, but it should be stated in the caption. What does the size of the circles in this figure

mean? Is it just proportional to the value of the correlation? That information should be in the caption.

The information was included in the caption:

"The x's in the figure represent not statistical significant correlations at $p < 0.05$. The size for the circles is proportional to the correlation values"

P11-L22: moNths. The 'N' is missing

Thank you. It has been corrected

P12-L8-10: The description of what is shown in figure 7 included in the main text does not coincide with the caption of the figure. The caption seems to be wrong. Please, revise it.

The caption was wrong and it has been modified:

"Monthly percentage of occurrence for every WT associated with moderate, severe and extreme dry conditions. The red bars represent the number of times that each month was affected by each drought category"

P13-L3: Figure 7: Why are percentages negative? What WT is the one with a positive paper percentage? Its color is not included in the WT color table. Please, revise the caption and the main text and include this information.

The negative sing is a mistake in the figure, it was deleted. The original positive value (red bars) does not represent any weather type, just the number of times that each month was under different drought categories. It has been clarified in the text and in the caption.

P13-L10: Both the main text and the caption of Figure 8 should explain what the authors mean when they talk about the onset, the peak and the termination of the drought episodes. I guess the "onset" is the month in which the episode begins, the "peak" is the month in which the episodes reach the highest value of SPEI1 and the 'termination' is the month after the month in which the episode ends. But these ideas are not clear in the main text or in the caption.

*The description of onset, termination, and peak and other important terms were now introduced and best explained into the text accordingly with your suggestion. And yes, in this table the **onset** and the **end** represent the first and last month of the episode and the **peak** is the highest SPEI1 value reached in the episode.*

P19-Figure 10 caption: Correlations shown in figure 10a are obtained from monthly series? What are the 'X's in figure 10b? Confidence level?

Monthly time series were used to obtain correlations in Figure 10. The x's represent not significant correlation with 95% confidence level. This information was included in the caption.

Figures 10 and 11 Captions: SPEI1 is enough, delete '...the 1-mo Standarised Precipitation-Evapotranspiration Index...' (idem in figure 11 caption)

Changed

P19-L25 to P20-L2: The interpretation of figure 11 is already included in the figure caption. It could be deleted from the main text.

It was deleted from the text.

P21-L19: Should it be '...increased with the TEMPORAL SCALE OF SPEI...'

Changed

P21-L20: 'Figure 12b and c...' should be a new paragraph

Changed

P21-L31: '...basin features and water REGULATION...' this point is very important in the interpretation of the results. Nowhere in the paper it is said whether the streams were regulated or natural. I guess they are regulated and, thus, it is difficult to obtain clear interpretations from them.

We understand your concern and we confirmed that on the Spanish part of the MLSHD exist more than 2000 dams (according to the Hydrographic Confederation: <https://www.chminosil.es/es/>). In the Portuguese part are also several dams and hydroelectric stations. For this reason, we removed the analysis of the SSI. Although, Añel et al., (2014) found the dams in the Miño-Sil river basin had no influence on the natural river flows over the period 1978-2012 and based on this report we performed the initial study with the SSI. (<http://catedranaturgy.webs4.uvigo.es/content/Workingpaper.pdf>)

The same analysis was performed but utilizing datasets of runoff at a resolution of ~ 4 km to compute the Standardised Runoff Index (SRI). An explanation of this index was added in section 2.

P22-L13-14: I do not understand very well what authors mean with their sentence 'The results revealed the frequency of the WTs prone...' Please, revise.

The sentence was changed

P23-L3-8: As I already said, I do not think any conclusion can be obtained about the influence of AMO.

After considering your concern, we agree, and the analysis of the AMO was removed from the manuscript.

P23-L10-14: Conclusions about soil moisture are sound, but results about river flows are much more dubious since those flows are most probably regulated.

Due to the resolution of the soil moisture data (0.25°) we decided to utilize another source of soil moisture. In this case as commented before we utilize the soil moisture from Terraclimate, which has a resolution of ~ 4km and is consequently more representative for the region. We used here the Standardized Soil Moisture Index (SSMI) (Hao and AghaKouchak, 2013)