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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Meteorological drought in the Miño-Limia-Sil hydrographic demarcation: The role of atmospheric drivers" *by* Rogert Sorí et al.

Anonymous Referee #2

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Review of the manuscript nhess-2019-314, entitled "Meteorological drought in the Miño-Limia-Sil hydrographic demarcation: The role of atmospheric drivers", submitted by Rogert Sorí et al. for possible publication in Natural Hazards and Earth System Sciences.

Recommendation Sorí et al. assessed drought characteristics in the Miño-Limia-Sil basin (NW Iberia) from 1980 to 2017 using the Standardized Precipitation Evapotranspiration Index (SPEI). The temporal variability of drought metrics was linked to changes in the dominant weather types and atmospheric circulation patterns in the region. Weather types in the study domain were classified using an automated version

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of the standard Lamb weather types' scheme. The study is interesting from the climatological point of view and falls within the scope of NHESS. The level of innovation of this work is fair/reasonable. Albeit with the availability of several studies, which assessed drought characteristics in the IP (most of them are referenced herein), this study applied well-established/existing methods in a standard fashion for a cross-boundary basin in the IP. The manuscript is generally well-written. However, the manuscript cannot be accepted for publication in its current version. Some methodological issues should be clarified. The structure of the manuscript should be refined. My major and specific comments are listed below:

I. Major comments - I want to point out one smaller aspect that deals with the general readability of the text. The readability is sometimes hampered by the use of various abbreviations (e.g. P, Y, M, MSRB, LRB, MLSHD, NWIP, VIMF, etc). If the reader is not very familiar with the terms and abbreviations, it is hard to follow. I would suggest using only those abbreviations that are reoccurring and central for the topic and trying to avoid others. This would strongly improve readability. - In the methods section, the authors need to clarify the final number of weather types retained in their work. Have they applied any regrouping/reclassification of hybrid types? What are the stopping criteria of their classification scheme? What statistical significance criteria are applied? How robust and reproducible are the results? What guided the reduction and grouping? Can you illustrate what the types represent, via typical flow or MSLP patterns? Also, a map illustrating the 16 points used for weather type classification should be included. - I am wondering why the authors have not used some Mediterranean specific indices, such the WeMO and MO. Also, I am wondering why the authors did not consider SST as a driver of drought variability in their domain. This can be implemented using El Niño 3.4 index (SST anomalies in the central Pacific), El Niño 1 + 2 index (SST anomalies in the eastern Pacific) or SST anomalies in the tropical Atlantic. I am aware that the manuscript focuses mainly on specific atmospheric drivers, but at least in the discussion, the authors need to highlight the possible role of SST warming in drought reinforcement. See, for example: https://doi.org/10.1175/JCLI-D-11-00296.1 - The au-

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thors should discuss their results in the context of some earlier studies whose results contradict the findings of the current study (particularly with respect to the significant role of NAO in drought variability in Europe in general and the IP in particular). See for example: https://doi.org/10.1007/s10584-007-9285-9. 10.1007/978-94-007-1372-7_3 - The authors focused mainly on the temporal variability of drought and its connections with WTs and climate teleconnections. However, I have not seen any attempt to show the varying spatial response of drought to these drivers. The reasonable spatial resolution (0.1°) of E-OBS allows for a reliable assessment of the spatial variability of drought in response to the different atmospheric configurations. The authors indicate in the abstract "We concluded that regional patterns of land-use change and moisture recycling are important to consider in explaining runoff change, integrating land and water management, and informing water governance". I think decision-makers and water resources planners in any catchment seek for detailed information on the spatial variability of droughts so that they can adopt integrated policies and strategies for managing their catchment, taking into consideration the different conditions at both upstream and downstream. - In the Introduction, the authors lack the opportunity to comprehensively provide evidence on the hydrological, environmental and socioeconomic importance of MLSHD in the IP. - A short description of the study domain, highlighting its main physiological, climatic and hydrological settings is needed. Section 3.1 is misplaced in the results section and should be forwarded to a new subsection called "study area". - A justification of the selection of the (-0.84) as a threshold for defining drought events is needed. - In the methodology, the authors should clarify how the different drought metrics were computed? How were the trend and its statistical significance assessed? Have they accounted for the possible presence of serial correlation in the series? - The authors should clarify the rationale behind constraining their study from 1980, while E-OBS dataset extends back to the 1950s (probably not for all climate variables). - I would recommend adding a new figure, in which the authors compare the accumulated SPEI values corresponding to each WT. - Prior to calculating correlation, it is important to detrend the series of the frequency of WT. - In the discussion, the authors should refer

to the role of zonal and meridional circulation in drought characteristics (in the context of the directional WTs). - It is recommended to add a table, which summarizes trends of the 10 main WTs over the study period, their statistical significance and compares those with the trends observed for the different drought metrics (e.g. drought, severity, and occurrence). - HS method for calculating PET is a temperature-based method, which is more suitable for arid and semi-arid regions (not humid regions like the study domain). Have the authors tested the performance/accuracy of this method in their region? Several studies reported a less performance of temperature-based methods in assessing PET in humid climates. - Given the limited area of the study domain, the spatial resolution of SMroot data seems to be coarse ($0.25^\circ \times 0.25^\circ$) to provide a reliable assessment of the response of soil moisture to precipitation deficit. Also, in humid climates like those of the study area, the response of soil moisture to accumulated precipitation deficit is more pronounced at longer time scales (not 1-month time scale). The persistence of negative soil moisture anomalies is expected to be higher when there is a cumulative long-term decrease in the amount of precipitation. This aspect should be discussed thoroughly. - Describe all symbols given in Eq. 1. - Which index exactly of the NAO, as well as ENSO, was used? Please, be more specific. There are different indices for quantifying each of them. - Section 2.5 should be placed earlier in the materials and methods section (before the description of drought calculation). - The role of aerodynamic components in drought evolution should be discussed, given that these influences are not considered in HS method.

II. Minor comments - Title: It is recommended to indicate the location of the study domain (i.e. NW Iberia), as the majority of the NHESS readers are not familiar with the study basin. Also, it is important to include “hydrological droughts” in the title. P1 - L14 and other parts of the ms: “period of” <> “period”. - L16 and other parts of the ms: “mo” <> “month”. - L17: For a study that covers 38 years, the use of the term “historically” is misleading; please, define the confidence interval at which the significance was assessed. - L18: “different” <> “the different”. - L19: Based on which scheme this classification was made? The abstract should stand alone based on this basic informa-

tion. - L20: “were directly related to dry and wet conditions” This statement is vague, with no clear phrasing. It does not make a clear conclusion on whether these weather types are favoring for above-normal or below-normal precipitation. - L25: Please, define the rainy season. - L27 and other parts of the ms: “1 y” <> “1 year”. - It is unclear how meteorological droughts assessed at 1-month time scale can be linked with land use changes (which almost occur at a coarse temporal scale”.

P2 - L5: Please, give some examples of these thermodynamic factors (e.g. wind speed, air pressure). - L15: Delete “e.g.” - L19: “the precipitation” <> “precipitation”. - L25: “land” Do you mean air temperature? LST has a different conception and is mostly assessed using remote sensing products (e.g. MODIS, AVHRR), which are only available for the most recent decades. - L28: The study of Vicente-Serrano et al. (2011) does not provide any assessment of future projections of precipitation P3 - L8: “a homogeneous region in terms of the total P variance over the IP”. This statement should be elaborated thoroughly. P4 - L25: “A drought episode was considered to occur when the SPEI at the temporal scale of 1 mo fell below zero, reached a value of at least -0.84, and later returned to positive values”. This definition should be made simpler.

P5 - L1: “Results” <> “Results and discussion”. - L9: Language and style should be revised. - L12: “modulate” <> “impact”. - It is unclear why the classification of weather types is only restricted to the period 1989-2017. - L15: Please, define this spatial window. - For classifying weather types, the authors should clarify how SF, WF, ZS, ZW, F, and Z were computed?

P6 - L20: “from daily values” <> “aggregated from daily values”. - L21: The name of the station “Albufeira Do Alto” does not fit with that labeled in Figure 1.

P7 - L5: “the annual cycle” <> “the year”; “western” <> “the western”. - L20: What is the difference between “extensive” and “intense”? Do you refer to drought duration and severity?

P8 - L30: “for in” <> “for”. - The acronyms “WTs” and “CWTs” are used interchangeably

in the text. - P23 (L10): “the soil” <> “soil”. - P23 (L10): “the moisture” <> “moisture”. - This work emphasized that drought did not respond linearly to most of the dominant circulation patterns in this region (apart from SCAN, AO) at 1-month timescale (Figure 10a). This finding should be discussed thoroughly in the text and linked with available literature.

Tables - Table 1: There is a refinement of the drought categories of Agnew (mild drought is masked with another category). - Table 2: Trends in SPEI values should be expressed in z-units/year.

Figures - Figure 1: In the legend and caption, “rivers” <> “streams”. The negative symbol corresponding to the longitudes should be deleted, given that the direction “W” is already included. It is important to include a distribution of the meteorological stations whose data were used for SPEI calculation. - Figure 3: how were drought episodes defined? Have you applied n consecutive months with SPEI <-0.84? - Figure 4: I would recommend using the anomalies (not the actual values) of SLP corresponding to the different WTs. This will facilitate defining the positive and negative centers of action that control air advection at the surface. - Figure 5: The use of the symbol “x” should be described. The use of the legend in a vertical form is confusing, given that all WTs at the top of the panel show a negative correlation (shown in blue). I would recommend reversing the legend so that negative values of correlation are shown at the top, while negative correlations are illustrated below. Why the authors did not use a portrait diagram showing the interpolated surface of the correlation coefficient, with some contour lines to show the significance of the correlation? This will facilitate the readability of the figure. - Figure 6: I would recommend plotting the events at the x-axis, while the stacked bars show the contribution of each WT to such events. This will deliver the message clearer. - Figure 7: why the percentages are given in negative? To which WT refers the “red” color? I would recommend adding a column to the three drought categories, which refers to wet conditions (i.e. SPEI values >0). This contrast can show interesting results about the role of each WT during dry vs. wet conditions.

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