

Authors reply to anonymous referee #1 are provided point by point in blue characters.

Comment on nhess-2021-41

Anonymous Referee #1

Referee comment on "Assessment of centennial (1918–2019) drought features in the Campania region by historical in situ measurements" by Antonia Longobardi et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-41-RC1>, 2021

Title: Assessment of centennial (1918–2019) drought features in the Campania region by historical in situ measurements

Author(s): Antonia Longobardi et al.

MS No.: nhess-2021-41

MS type: Research article: First review

Special Issue: Recent advances in drought and water scarcity monitoring, modelling, and forecasting (EGU2019, session HS4.1.1/NH1.31).

Summary

In this manuscript, the authors explore the Centennial (1918 – 2019) drought features in the Campania, region (southern Italy) by performing a trend analysis using the Modified Mann Kendall test (MMK) and characterize the multi-scale SPI index combining with the Run Theory.

This paper does several things that make it a novel and timely contribution of broad interest to many communities (including the hydrologic, hydro-climatologic, and water resources) and a good fit for NHESS, with its integrative perspective as a journal. The overall context of the subject seems to be appropriate for this journal.

Although the number of regional and continental-scale studies of drought have increased over the last decade, too few exist for many locations in the world to make these results accessible and viable for use by climatologists and water resource professionals. This paper helps ameliorate this issue for a large region of the Mediterranean region (Italy).

A second contribution of this work is that it presents a flexible methodology for others to follow to increase the number of regional to continental scale interpretations of drought

Thus, the importance of this paper is that it demonstrates a new method by using the RUN theory approach for exploring, interpreting historical data; and to recognize the feature of droughts such as duration, severity, intensity, and interarrival in order to address water resources and climatic assessments of extreme events such as meteorological droughts for others to follow. The manuscript is also well structured and written. I would suggest to publish the paper after some minor revisions.

Authors would like to thank the anonymous reviewer for his/her encouragement in the research idea and his/her helpful comments which resulted in an improved version of the manuscript. A point by point reply to the comments is in the following.

General comments

O) To be more concise and precise, I suggest the authors better to add "southern Italy" to the title to be direct to the point and reflect the geographical location of the study area like this: **Assessment of centennial (1918—2019) drought features in the Campania region by historical in situ measurements (southern Italy)**

Authors thank the reviewer for his/her useful suggestion. The title of the revised version of the manuscript includes it.

- **I)** In the abstract, introduction, and the conclusion, respectively the authors stated that:

P.2, line 12-13: " understanding historical drought conditions in this area is necessary to plan mitigation strategies to further face future climate change impacts".

P.3 Line 44-45: "the southern Europe regions indeed, thus historical in-situ long term measurements are crucial for understanding historical drought conditions and to plan mitigation strategies to face future climate change impacts".

P.20, Line 300-301: "The reported research illustrated how historical in-situ long term measurements are crucial for understanding historical drought conditions to plan mitigation strategies to further face future climate change impacts".

I-a) The statements listed also seemed to be redundant in the paper, expressing the same similarities and are not conclusive. I would suggest rephrasing or remove this sentence in one section. It would make it easier to read.

I-b) For instance, How is the statement "The reported research illustrated how historical in-situ long term measurements are crucial for understanding historical drought conditions to plan mitigation strategies to further face future climate change impacts" justified?. However, I suggest to strengthen the study motivation here with more details on the value of historical data or analysis.

The sentences just mentioned by the reviewer were meant, by the authors, to stress the importance of observed data. The idea is that understanding historical droughts does not represent a value by itself but by the possibility we have to learn from the history about how the region would respond to a precipitation shortage period, how severe would be the response and how quickly (or long) would it take to recover. These information are important to set drought forecasting strategies which could help in preparedness actions,

planning mitigation and adaptation strategies to projected climate changes. As suggested, the sentences been reformulated in the introduction section. Please see lines 39-47.

- **II)** The SPI indicator at a given location can be calculated for different temporal scales. Here, six-time scales, i.e. 3, 6, 12, 24, 36, and 48-month, were considered. Why? Is 48 months long enough to capture also long-term variability of meteorological drought in the Campania region?

Yes, SPI48 is still an interesting indicator, especially for what concerns the water resources management sector. Because of typical geological properties, many groundwater systems are impacted by precipitation on very long temporal scale indeed. The important climate inter-annual variability makes furthermore worth to explore accumulation over long temporal scales. A comment has been provided in the revised manuscript version. Please see lines 235-237.

III) As far as I know, the classification of drought is also a challenging task. Therefore, a validation of SPI or another drought indicator is challenging. However, I guess it is not enough to evaluate an indicator without an exhaustive comparison with other indicators (multiscale indicators, SPEI, PDSI, among others). The main criticism of the SPI index is that it only integrates the effect of precipitation on drought, while other driving factors such as air temperature, wind speed, evapotranspiration, and soil moisture are not taken into account. Consequently, the advantage of using "SPI" is a little unclear in your paper. We can understand that the SPI index would give a robust estimate of wet/dry conditions (as described in the paper), however, it is still unclear why SPI index is chosen given its limitations.

Would be helpful to give some more information about the criteria used to select the SPI indicator to explore the drought event. The response to this comment is not mandatory, however, this information would be helpful for the reader's understanding of the characteristics (limitation) of SPI index.

Authors are aware of the limitation in the use of a single index drought assessment and, at the same time, of the limitation itself of the SPI index. However, the World Meteorological Organization has recommended that the SPI be used by all National Meteorological and Hydrological Services around the world to characterize meteorological droughts (World Meteorological Organization, 2012). Being the presented research a rather detailed focus on an area suffering from data availability and analysis, the authors found worth to take a start from a general broadly accepted methodology.

- **IV)** Statistical tools (Application package software), that authors used in the study are not well defined in the present paper. I encourage the authors to cite them.

Authors have developed ad-hoc script and function in Matlab and R to run the statistical tools.

Minor revisions

The paper has a few inconsistencies in terms of citation style. Please, check all the citations to make sure that e.g. Authors et al. (2020), (Authors et al., 2020), and so on

are used in a consistent way. This will improve the readability of the manuscript! Some examples are listed in **the technical comments (See page 4)**
Authors apologize for this. Citation's style has been checked in the revised version of the manuscript.

P.5, L97-107: How many precipitation gauges were used in the whole area to interpolation approach? Please, specify the sources of data in the dataset section (Meteorological offices?).

For the period from 1919-1999 data from 154 stations were available, whereas from 2000-2019 data from 187 station were available. Please see lines 103-106 of the revised version of the manuscript.

P.5, Line 95: The map figures (Figure 1) are not up to publication standards. It is hard to read. The caption is unclear. They need to be improved (Fig1, Left panel), resized (Fig1, Left panel), and projected with latitude and longitude coordinates (Right panel: the Italian peninsula).

Figure 1 has been changed in the revised version.

P.5, Line 95: Based on the caption (Fig 1, Middle panel) the title of the map (Middle panel) should be written in English.

Figure 1 has been changed in the revised version.

P.9, Line 195: Furthermore, based on the caption of Fig 2, the label of the y axis should be "time". Please, add the label to be more clear.

Figure 2 has been changed in the revised version.

P.10, Line 210-213: The following paragraph should be integrated with the 2.4 Trend analysis section, not in the Results and discussion section:

"The reason for the use of the Modified Mann-Kendal test (MMK) lays in its accuracy for the analysis of correlated data (Hamed and Rao, 1998; Mondal et al., 2012; Sa'adi et al., 2019), which is the case for the SPI time series in this study, compared to the original Mann-Kendall trend test without any loss of power"

The paragraph has been moved to the "trend analysis" section as suggested by the reviewer in the revised manuscript version (please see lines 169-172).

P.8, Line 179; P13, Line 239: The title of the related 2 sections (2.5 Drought characteristics, 3.2 Drought characteristics) is confusing regarding the content. It's a little bit difficult to see the difference between the first title (2.5 Drought characteristics) and the second (3.2 Drought characteristics). It's the same!. Better to re-write this second title to be direct to the point and reflect the objective of the section.

The title of section 3.2 has been changed in "Drought characteristics assessment in the revised manuscript version.

P.15, Line 275-276: The authors stated that: "The SPI₁₂ represented a neutral condition, with a very important spatial variability of peak conditions. It is not clear sentence. The authors should expand their explanation.

Thanks for this suggestion. Please see lines (301-302) of the revised manuscript version.

P.3, Line 41-46: The same idea is rephrased twice in the section. Consider re-writing this paragraph to avoid repetition and to improve clarity.

Probably the reviewer refers to pag 2 line 41-46. The idea has been revised.

Technical comments

A couple of examples of typos that need correction:

P.4, Line 84: The Campania region is located in the southwest of Italy (not in southern Italy) and occupies an area of about 13600 km² (not 14000 km²). Please, check this information.

P.4, Line 86: Replace "2000 m.m.a.s.l " by "2000 m a.s.l. " (metres above sea level (m a.s.l)).

P.5, Line 105: Cite one spatial resolution is enough (0.09°x0.09° or 10 x 10 km).

P.7, Line145: Should be: "...for non-parametric Modified Mann-Kendall (MMK) and Sen's test approaches".

P8, Line 180: Should be: "To describe meteorological drought features of the studied area, the occurrence of drought events was evaluated for each cell of the gridded dataset according to the SPI threshold, and...".

P.10, Line 209: should be: "The Modified Mann-Kendall (MMK) test and the Sen's slope estimator were used to investigate temporal trends"

P.10, Line 211: Should be: "Modified Mann-Kendall".

P.12, Line 231: Should be: "the MMK test".

P.19, Line 308: Should be: "Modified Mann-Kendall".

Please, check all the citations to make sure that e.g. Authors et al. (2020), (Authors et al., 2020):

P.3, Line.36;

P.3, Line 36: No entry in the references for (Change, 2014).

Technical comments have all been fixed in the revised manuscript version.

P8, Line 169.

P.9, Line 195;

P.9, Line 186;

P.19, Line 309-310;

P.69, Line 69.

Please also note the supplement to this comment:

<https://nhess.copernicus.org/preprints/nhess-2021-41/nhess-2021-41-RC1-supplement.pdf>

Authors reply to anonymous referee #2 are provided point by point in blue characters.

Review of nhess-2021-41

Anonymous Referee #2

Referee comment on "Assessment of centennial (1918–2019) drought features in the Campania region by historical in situ measurements" by Antonia Longobardi et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-41-RC2>, 2021

General comments

The manuscript presents an analysis of droughts based on a 100-year long SPI series computed from observed precipitation series. It certainly fits within the scope of the journal and the SI. Overall the manuscript is adequately prepared, even though it needs to be carefully reread as it presents many typos and sentences that can be improved in language (referee #1 has already provided a detailed list of these technical corrections to be made). I have some concerns on the framing of the manuscript and on the methodology. Also some conclusions on the spatial pattern may benefit from a more in-depth discussion. These points are better detailed below. For these reasons I suggest major revisions prior to publication of the manuscript.

Authors would like to thank the anonymous reviewer for his/her encouragement and his/her helpful comments which resulted in an improved version of the manuscript. A point by point reply to the comments is in the following.

Specific comments

Introduction: One focus of the manuscript are the historical trends of droughts (in terms of SPI), which may be related to climate change. The authors mention climate change, but I think that more words should be spent on this issue, considering the growing literature on the subject (e.g.: DOI: 10.5194/nhess-20-3057-2020, <https://doi.org/10.1016/j.scitotenv.2020.140094>, <https://doi.org/10.1029/2020EF001502>)

Climate Change was not truly the focus of the presented work and this is why authors did not emphasize this concept in the original manuscript version. Also provided the comment of the reviewer 1, authors decided to at least better frame the specific issue in the introduction of the revised manuscript version. please see lines 39-47.

Section 2.4 The authors employ the modified version of Mann-Kendal trend test because SPI series are autocorrelated. If they would have referred simply to precipitation the standard test could have been applied. I think the authors should add the analysis of trends computed directly on seasonal and annual precipitation, and compare the results with those obtained with SPI (e.g. SPI3 and SPI12). Then the authors should comment on how significant are the differences between the two.

In the recent past of the literature about climate data trend, the Modified Mann Kendal test is getting wider application (Darand, M, 2020; Salman, S.A. et al, 2019; Ahokpossi, Y., 2018), also for precipitation time series. Additionally, for the specific case study a comparison in the application of the MK and MMK tests was studied in Longobardi and Boulariah (under revision) and the results provided no significant differences.

Temporal analyses of the precipitation regime of the studied region were already presented in a number of previous and currently under review studies (Longobardi and Villani, 2010; Longobardi and Mautone, 2015; Longobardi et al., 2016; Longobardi and Boulariah, under review). In particular the annual, seasonal and monthly scale regimes were investigated, as well as the intra-annual and inter-annual variability. Authors found redundant a repetition in

the presented manuscript, but a comparative discussion of the results found for the SPI temporal and spatial analysis in the light of the finding related to the temporal and spatial analysis of the precipitation regime, has been introduced in the revised manuscript version, please see lines 241-245.

Conclusions: the authors summarize the results obtained in terms of spatial patterns. Nevertheless, they do not provide any justification from a physical/climatic standpoint. I think the authors should at least try to speculate on this, also based on their possible knowledge of the reasons (e.g. orography) of precipitation patterns in Campania.

The orography of the region impacts the precipitation spatial pattern but also the precipitation variability spatial pattern. This issue was already included in the original version but it has now been stressed in the revised manuscript version. Please see lines 223-225, 247-251, 281-283, 289-291, and more importantly the revised conclusions lines 347-354.

Fig 1 (this relates also to the previous point). I see that the gridded dataset derives from a ground based network that has a quite variable density of gauges. A discussion on how this may impact their study and in particular the conclusions on spatial patterns should be added.

As added in the description of the case study of the revised manuscript version, the two rain gauge dataset merged in the current analysis have rather similar density and are almost similarly distributed over the region, with an exception for the central coastline area of the Peninsula Sorrentina. This issue was not considered in a proper analysis, but the rather similar spatial features of the dataset should have not strongly affected the interpolation analysis. Please see line 103-106.

Again for the Conclusions (or for Results section), perhaps the authors should discuss how their findings relate to previous studies at the European scale: DOI: 10.1007/s11269-012-0177-z, DOI: 10.1002/2013GL058573, DOI: 10.1002/joc.6719, ...
Authors thank the reviewer for his/her suggestion. This would be considered to discuss the long term SPI behaviour. Please see lines 211-218 of the revised manuscript version.

Minor points

Fig. 4, 5, 6, etc. (all maps): perhaps maps could be improved with a background shape of Campania and the parts of the neighboring regions of Italy

Fig. 7 Perhaps could be presented as a map as well. If the authors would like to keep this differentiation, the plot should be however improved (for instance use a piece-wise line instead of a spline)

L159: σ and ρ seem undefined

L172: Perhaps the pairs are σ_1

L175: T_i , should be \bar{T}_i

L187-188: These equations are unclearly written. Also keep in mind that Author guidelines discourage to denote variables with multiple letters.

As I have written in the general comments, there are many points where the language needs to be fixed. Instead of giving a list of corrections, I suggest the authors to carefully reread the manuscript and eventually take into consideration the aid of a native speaker.

All of the issues referring to minor points, figure quality and formatting, equations formatting and explanation have been addressed in the revised version of the manuscript.