

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

## **Review of nhess-2021-41**

Anonymous Referee #2

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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

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### **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.

### **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.*

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. The reason for the assessment of the SPI index is based on the fact that 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), but actually the authors did already investigate, in the recent past, the temporal pattern of seasonal and annual precipitation for the same region. A comparative discussion about the two study (precipitation trends versus SPI trends) is foreseen in the revised manuscript version.*

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 definitely impact the precipitation spatial pattern but also the precipitation variability spatial pattern. This issue will be better addressed in the revised manuscript version.*

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.

*Yes, also according to the comment of the reviewer 1 this issue will be addressed in the revised manuscript version.*

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 help improve the discussion of the results in the revised manuscript version.*

### **Minor points**

Figg. 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:  $\rho$  and  $p$  seem undefined

L172: Perhaps the pairs are  $\rho_i$

L175:  $T_i$ , should be  $T_i$

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 minor points, figure quality and formatting will be addressed in the revised version of the manuscript as well as a mother language revision of the manuscript is foreseen.*