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Comment on nhess-2022-260

Anonymous Referee #2

Referee comment on "Multi-scale EO-based agricultural drought monitoring system for operative irrigation networks management" by Chiara Corbari et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-260-RC2, 2023

Review of "Multi-scale EO-based agricultural drought monitoring system for operative irrigation networks management"

This study proposes a methodology to assess drought conditions in two irrigation polygons of Italy based on different data sources obtained from satellite data. My opinion is that this manuscript should not be published because it is affected by several formal and methodological problems. The methodology is not well explained and justified and in general all the manuscript it is very difficult to follow. The use of different data sources of different origin makes difficult to know the connection between meteorological and agricultural droughts. The results are also presented in a very confuse way, with different plots in which it is not possible to obtain a clear message about the relationship between metrics and the evolution of the existing anomalies. Below I am providing specific comments that support my general assessment and my suggestion to reject this manuscript.

9-14: Very confuse summary of the results. What is a cumulative drought monitoring system? A drought index can be cumulative, but I wonder what are the authors referring in relation to a drought monitoring system. It is not clear what kind of correlation the authors are referring.

17: There are much better references to refer to drought characteristics and impacts. It seems that the authors have simply cited some papers related to drought… See e.g. IPCC AR6 Chapters 8 and 11 for a summary of drought complexity and implications.

23: In irrigated lands water shortage can be relevant but in rainfed agricultural areas precipitation (but also temperature and atmospheric demand) play a very important role.

25: Definitively this is not the best to refer to land atmosphere feedbacks and droughts. Again the citations are very poorly selected, which gives a very bad impression as it seems that references are only located randomly in the text to justify the use of references. About this topic, I would recommend to read Miralles DG, Gentine P, Seneviratne SI, Teuling AJ. 2019. Land–atmospheric feedbacks during droughts and heatwaves: state of the science and current challenges. Annals of the New York Academy of Sciences. Blackwell Publishing Inc., 1436(1): 19–35.

ttps://doi.org/10.1111/nyas.13912.

.25. It should be irrigated agriculture.

.28. I would say better: "during the dry season in water limited regions". I would not refer to specific regions.

.29. One-sentence paragraph? I also find this very disconnected of the context. I suggest to remove this sentence as it does not provide any relevant message.

.35. Again poorly and non-suitable citations. Vicente-Serrano 2006 analyses spatial pattern of meteorological drought but there is nothing on this study on the dynamic of different types of drought. The authors should revisit all the citations of the manuscript. The poor and unsuitable citation approach is a solid formal argument to suggest the rejection of the manuscript.

.40. Cite the WMO guidelines for SPI in which it is recommended as a reference drought index.

.45. The SPEI is perfectly comparable in time and space (as the SPI) Also the Standard Palmer Drought Index is perfectly comparable spatially, so the argument of the authors is not correct. Why is the use of potential evapotranspiration a limitation? I would say that given atmospheric evaporative demand has a relevant influence on drought severity it should be an advantage.

48-56: If remote sensing soil moisture is affected by so large uncertainties, what is the justification of its used? The low correlations found among soil moisture datasets presented below even justifies more my assessment.

.57. land surface temperature has been widely used. See e.g. TCI developed by Felix Kogan and the drought monitoring systems (and studies) that use it.

.71. The optimal solution is really to relate drought objective metrics with impacts and then select the most suited approach. For this purpose, empirical analysis that relates drought indices and impacts is needed.

.83. I wonder if the authors are proposing a drought monitoring system or a drought index. I believe that they are developing a drought index.

.91. A new drought index should be evaluated with impact data (e.g. crop damages and yields). The volumes of irrigation may be related to several other factors including water availability in reservoir storages, groundwater, etc.

.105. Crop yield is also constrained by VPD anomalies and increases in the atmospheric evaporative demand, particularly under low soil moisture conditions.

.106. Increase in crop temperature can be also caused by decreased leaf stomatal conductance as consequence of increased VPD.

.115. Are the different variables following a normal distribution in order to apply this equation?

.117-120. It is confuse if the authors are using the monthly or daily scales.

.124. Figure 1 is confuse. It is not clear how the different indices are merged in order to generate the ADMOS. What is the criterion followed to select the thresholds?

.130. Are equations 2 and 3 necessary? I do not think necessary to include the equation of the Pearson's r statistic.

.142. was affected? As the sentence refers to 2012 I think better use the past. Same 143.

.159. How robust is the calculation of SPI and the other drought indices based only on 20 years of data? e.g.in 168 in is indicated that 13 years of data are used. This will provide very uncertain indices. WMO recommends at least 30 years.

Section 22.3. It is very confuse how all these soil moisture indices of different resolution and time span are used together. There is not explanation and justification of why these different soil moisture products are used and what is the advantage of using different datasets if they show low agreement.

.214. Why thermal bands are resampled to 100 meters?

Figure 3. It is impossible to identify the drought periods according to the SPI based on this plot. I would suggest to be replaced all the plots by time series.

Figure 4. Same that for precipitation. I do not think it is possible to compare these different datasets based on these plots. The statistics that compare the datasets suggest strong uncertainties and difficulties for comparison. I do not think that the authors are providing realiable combination of the different datasets and, in addition, validation is not provided.

Same comments are valid for surface temperature and vegetation indices. My impression is that authors have used all the information they have found by different sources, but they have not considered any coherent approach to analyse drought severity, to validate the different products and to stablish uncertainties associated to the datasets. In addition, the information is not showed in a coherent way and it is very difficult to determine the evolution of the anomalies in the different metrics and also to establish comparisons.

Figures 9 and 10: Based on the uncertainties in the datasets and methods indicated above, the uncertainty in the results described based on these figures are very strong. It is not possible to infer on which dataset (e.g. soil moisture, surface temperature and vegetation index) this plot is generated.

.445. I cannot identify how the different products are combined in order to generate the ADMOS and it is very confuse the use of different data products at the same time and in an independent way.

.545: I agree that different indices are compared, but this is not done in this study. There is not validation of different metrics and selection of most suitable according to empirical information.

.549: But the remote sensing information is not used in a coherent way considering a careful validation. Several datasets are put together considering different time periods and I cannot find a coherent message by so confuse merging.