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Comment on hess-2021-230

Anonymous Referee #2

Referee comment on "Evapotranspiration enhancement drives the European water-budget deficit during multi-year droughts" by Christian Massari et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-230-RC2, 2021

Review of "Evapotranspiration enhancement drives the European water-budget deficit during multi-year droughts". This is an interesting study focused on a very relevant research topic as availability of water resources is essential during dry years and determining the possible role of ET is particularly important during these periods. The study covers catchments in different regions of Europe and related streamflow and precipitation observations. I find two main issues that prevent for a positive assessment of the ms. The first one is related to the null consideration of land cover changes and human management to explain the different changes between meteorological and hydrological droughts, and the second one the consideration of multiannual meteorological droughts as driver of hydrological droughts since the majority of the small catchments analysed should be insensitive to long time scales of meteorological droughts. I think the authors should be carefully consider these two issues in order to provide more robust results on this very relevant topic.

Line 37. I would not say runoff response to precipitation changes has been rarely studied. There is a vast scientific literature on this issue considering both modelling and empirical studies. A quick search in Scopus with (TITLE-ABS-KEY (runoff) AND TITLE-ABS-KEY (precipitation AND change)) returns more than 8000 documents so I think authors should reformulate this sentence.

Line 39. It should be Van Loon

Introduction in general. What I miss in this section is a particular focus of the role of land cover and human management/demand. Vegetation characteristics and land cover changes are primary drivers of the enhancement of hydrological droughts during periods of precipitation deficits (see e.g. https://www.nature.com/articles/s41467-018-06013-7). The partition of precipitation between blue and green water may be strongly relevant during periods of water deficits in which water consumption by vegetation would enhance, reducing runoff production. For this reason, vegetation changes may be determinant and much more important than changes in temperature and demand, ultimately affecting ET. This has been observed for example in Mediterranean catchments (https://www.sciencedirect.com/science/article/pii/S2213305421000321#!). The other important factor is human demand, which represents higher percentage related to the total available water during dry years, exacerbating hydrological droughts in highly regulated basins (https://www.sciencedirect.com/science/article/pii/S0341816216301291?via%3Dihub). I agree with authors that enhanced temperature and atmospheric demand increase ET, and this is particularly relevant during dry periods. Thus, certain role has been already identified in Mediterranean areas (https://iopscience.iop.org/article/10.1088/1748-9326/9/4/044001), nevertheless, the strong increase of hydrological droughts observed in large regions of southern Europe cannot be explained exclusively by climate/runoff relationships. The problem is much more complex and climate/land cover changes/water regulation and demand should be considered in this kind of analysis to explain why hydro droughts are exacerbated in relation to precipitation deficits (see https://www.sciencedirect.com/science/article/pii/S0012825211000134).

Line 70-75: See limitations of e-obs in some regions of Europe in which this dataset reinforces artificially negative precipitation trends (https://iopscience.iop.org/article/10.1088/1748-9326/ab9c4f). Note that this data does not include homogeneity testing and it may have limitations in areas in which low percentage of meteo stations is used (e.g. southern Europe).

Lines 77-83. Note the problems of the potential evapotranspiration product in ERA5 (https://confluence.ecmwf.int/pages/viewpage.action?pageId=171414970). I would recommend authors to calculate the atmospheric evaporative demand using the FAO-56 equation from ERA5 inputs instead of using the product in ERA5. It solves the existing problem.

Lines 85-87. If the purpose is to characterise drought events I would encourage authors to use more detailed temporal resolution. Annual data could mask dry conditions at the seasonal/monthly scales that could be strongly relevant to explain hydrological drought response to water deficits. In the introduction it is stressed the complexity of runoff generation processes and to restrict the analysis to the annual scale could limit the identification of snow effects, high precipitation events, etc., which are determinant to evaluate the factors of occurrence of hydro droughts.

Section 2.2. I see the authors have restricted the analysis to catchments below 50.000 km2 in Europe. Nevertheless, this does not reduce the role of possible human disturbances (e.g. highly regulated catchments in the headwaters). It seems that series with data gaps are used. I think this is strongly problematic to assess dry severity as high flows in one day that correspond to data gaps may entirely alter annual flow, particularly in small catchments of water limited regions. If the analysis are applied at the annual scale, why not to use directly monthly streamflow and then try to fill the gaps? Filling monthly data is less problematic than filling daily streamflow information.

Lines 124. Calculation of SPI is not correct. It must be calculated from monthly series and data must not be smoothed previously. Smoother series are obtained selecting long SPI time scales. There are WMO guidelines to calculate SPI. Drought definition is very arbitrary in the methodology and it strongly may affect the obtained results. Streamflow drought usually responds to varied time scales of meteorological drought according river regimes, catchment characteristics, climate, human disturbances, etc. Sometimes hydrological droughts respond to short time scales of meteorological droughts but in other cases they respond to longer time scales. There is a vast scientific literature on this issue that should be considered by authors (https://hess.copernicus.org/articles/20/2483/2016/hess-20-2483-2016.html, https://www.sciencedirect.com/science/article/pii/S0022169418308813, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2017WR022412). Ideally, the authors should consider to determine what is the time of response of hydro droughts to meteorological droughts and then to select the most suitable time scale for each basin. In any case, three years in meteo drought is a too long period to explain the occurrence of drought in small catchments, which usually respond to shorter time scales (http://www.int-res.com/abstracts/cr/v58/n2/p117-131/).

Line 185. The figure is not informative. In the majority of catchments analysed, different meteorological droughts have been recorded so the year of drought onset is not informative. Does it refer to the onset of the first drought identified? I do not think this information is useful to assess the characteristics of meteorological droughts in the region.

Lines 186-208: I think these results can be strongly affected by the proposed method (e.g. the selection of meteorological droughts based exclusively on multiannual information).

Lines 219-241: The spatial distribution of the mismatch between meteorological and hydrological droughts may be essentially explained by the role of vegetation changes and human water demand. The

Line 219. This method has not been previously explained. It should be detailed in the methods section.

Line 214-226. I mostly agree with these results, but the role of vegetation changes and human demand are determinant to explain the suggested temporal pattern. See e.g. https://hess.copernicus.org/articles/23/3631/2019/hess-23-3631-2019.html, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019GL084084).

233-241. I think that the annual focus is masking stronger influence of ET on runoff as authors are mixing the behaviour of the humid and dry season, in which the magnitude of ET is very different. I wonder why not to focus on the dry season independently in which most of the ET is recorded to identify this kind of impacts. Probably the role of ET would clearly reinforce in comparison to the annual resolution but mostly in energy-limited regions. In dry regions in summer it is expected small role of ET given the low water availability.

253-255. These are quite strange results. A reduction of precipitation by 95% should not increase ET, I would expect a reduction in ET as consequence of the very low water availability. In any case, a 95% of reduction seems to be too large to be reasonable.

263-265: It seems that small catchments selected for the Iberian Peninsula are located in mountain areas, and they are not water limited, by mostly energy limited so conclusions on this issue should be carefully reinterpreted. Probably the coarse spatial resolution of the data 36 km2 shows limitations to assess this kind of issues in complex mountainous areas of Spain. In any case, water demands by agriculture and vegetation changes are main driver in this area and I think that authors should consider these factors in some way in their analysis in order to obtain a reliable explanation of the observed changes. I wonder if in North Europe (e.g. UK water demands by urban supply during dry years) it may be also a substantial influence of external factors to explain different trends between precipitation and runoff during dry years.