Comments on "Historical droughts manifest an abrupt shift to a wetter Tibetan Plateau" by Liu et al.

This study analyzed the climate wetting/drying of the Tibetan Plateau from variations of historical soil moisture droughts over 1961-2014, focusing on the spatiotemporal patterns, long-term variations of soil moisture, and the related climate causes of summer (May–September). Multiple observation and reanalysis data were used for the analysis. To this reviewer, these analyses are very important to quantify the various aspects of the soil moisture droughts and their causes.

In reading through the manuscript, despite that fact that the authors have done a lots analyses, there seem some important technical issues that need to be considered carefully to ensure that the results of the study are reliable. Some of these issues are listed as follows:

1. The consistency of the used variables used in describing the water components. The study used different data sources for the analysis, e.g. the soil moisture is extracted from the GLDASv2.0/Noah dataset from the depth of 0-10cm, precipitation and air temperature from interpolation the gauge data provided by the Chinese Meteorological Administration, potential evapotranspiration, wind speed, radiation, vapour pressure deficit, latent heat flux, and net radiation flux again from the output and forcing datasets of GLDASv2.0/Noah. The authors need to consider if the relevant quantities are consistent before carrying out further analysis. Inconsistency in precipitation in the CMA data and that in GLDASv2.0/Noah can cause lots of inconsistencies in the derived soil moisture and its relationships to precipitation. See e.g. https://journals.ametsoc.org/view/journals/bams/99/2/bams-d-16-0074.1.xml for how to verify and ensure the consistency of the climate data records.

2. GLDASv2.0/Noah data is strictly not a climate data record, the authors need to verify the temporal consistency of the used variables to make sure that the trends in the used variables are true reflection of the actual states of Tibetan Plateau and not caused by e.g. the change of the forcing data. A suggestion is compare the relevant variables with the ERA5 data and discuss the uncertainties. Some comparison to in-situ observation should also help to ensure the validity of the conclusions. Examples of such comparisons could be for precipitation (e.g. those from CMA and input to GLDASv2.0/Noah), soil moisture and evaporation from in-situ observation and remote sensing, e.g. https://www.mdpi.com/2072-4292/13/18/3661/htm; https://www.mdpi.com/2072-4292/12/3/509, https://essd.copernicus.org/articles/13/3513/2021, among many others.

3. Changes in vegetation coverage is closely related to the changes in soil moisture and temperature, this seems completely neglected by the authors. See e.g. https://link.springer.com/article/10.1007/s10584-009-9787-8.

4. Figure 4 is rather difficult to comprehend, perhaps a Hovmöller diagram is more effective.

5. Trend lines and designated changes in Figures 5, 9 and 11 appear arbitrary, unless the authors can provide more details in trend detection that identifies 1995 as the year of abrupt change. Such a change in Fig. 5 is also not observed nor explained by the monsoon indices, AMO, PDO and ISM. These relationships should be explored more in detail. Does ENSO have any impact on the precipitation and circulation patterns? How about the solar cycles?