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Interactive comment on "Status of the Tibetan Plateau observatory (Tibet-Obs) and a 10-year (2009–2019) surface soil moisture dataset" *by* Pei Zhang et al.

Anonymous Referee #2

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OVERVIEW

The study describes the status of the Tibetan Plateau observatory and propose the development of a 10 years surface soil moisture dataset obtained from in situ observations.

GENERAL COMMENTS

The paper is fairly well written and clear. The described dataset is very important both for the relevance of the study area and the importance of obtaining long-term soil moisture time series to be used as benchmark for modelling and satellite products. However, I believe some points need to be addressed before the publication. I have

listed below my general comments with the indication of their relevance.

1) MAJOR: The description of the dataset needs some improvements. I am aware that the Tibetan Plateau observatory has been already described in several previous papers (Table 1 is great), but in the online dataset more details should be added (soil, land use, climatology, pictures, . . .).

2) MAJOR: I have found the paper a compromise between a data paper and a scientific paper describing and testing upscaling procedures. I believe that in the dataset all the upscaled soil moisture time series should be available. Indeed, I have found weak the reasons for selecting the arithmetic average as reference method (ground truth). I guess that its better performance is very related to the employed metric (CEC). Of course, a reference does not exist, and hence I would suggest to publish and to make the analysis (e.g., comparison with modelled data), for all upscaled time series.

3) MODERATE: The ATI upscaling method is not clear to me. Where did you get the data for computing ATI? Why is its performance so different from the other upscaling techniques? Why is the range of soil moisture with ATI approach much smaller than other approaches? Please, provide more details.

4) MODERATE: The results of trend analysis are not clear to me. What do the authors want to highlight with this analysis? Why showing both UF and UB metrics? Also, the interpretation of results is unclear. By looking at figures, the trend of SM and precipitation is not consistent for Maqu site (line 319). For the same site, positive and negative values of UF are present, therefore I would not conclude for a drying trend (line 338). All trends are not significant (see line 320) as UF and UB values are lower than critical values. All these points should be addressed.

5) MAJOR: I believe that the SM AA-valid time series is wrong and should not been used. Of course, by averaging different sensors varying over time is not a correct procedure. I suggest removing this time series.

6) MODERATE: In the results and discussion sections several small errors (typo) are present that should be corrected. A quick reread of these sections will fix these errors.

RECOMMENDATION

Based on the above comments, I suggest a major revision before the possible publication on Hydrology and Earth System Sciences.

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