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Hydrology and Earth System Sciences
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# *Interactive comment on* "A SMAP-Based Drought Monitoring Index for the United States" *by* S. Sadri et al.

**Anonymous Referee #2**

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The manuscript describes the use of SMAP soil moisture data products to generate a percentile-based soil moisture product for drought/pluvial monitoring. There is some interesting and highly relevant material here; however, as currently written, the manuscript lacks any firm conclusions and/or meaningful analysis. As a result, it reads more like a technical/progress report than an actual journal paper.

MAJOR

This shortcoming can be fixed by providing a more direct link between the (very interesting) "data adequacy" analysis presented in Section 2.3 and the presentation of index comparisons in Section 3. As currently written, the analysis in Section 2.3 reveals that the (current) 3-year SMAP data heritage is insufficient for a substantial fraction of CONUS. However, this "inadequacy" is never mentioned again in the paper and does



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not come into the analysis of results presented in Section 3 and discussed in Section 4. This is a real shame. At best, SMAP will last for 10 years; therefore, “data adequacy” will always be a pressing concern for the calculation of soil moisture climate percentiles.

Given this pressing need - how can the analysis in Section 3 be used to inform an interpretation of SMAP soil moisture percentile maps based on <10 years of data (e.g., as a tool for generating data quality flags, as a data mask or as a source of uncertainty information)? Does the fit between these new SMAP-based indices and existing drought/pluvial indices noticeably degrade for areas flagged as “inadequate” in Figure 5? Are there specific events there where the 3-year SMAP data record injects spurious percentile patterns into drought/pluvial events? If so, are the locations of these events adequately flagged as being problematic by results in Figure 5?

More analysis on these (and related) questions would greatly enhance the contribution of the manuscript (which currently is somewhat poorly defined).

MODERATE

1) Figure 2 – A major issue is calculating percentile products is always determining the seasonal intervals over which climate is considered stationary. Here, the authors choose to (implicitly) assume stationary climate within “hot” and “cold” 6-month portions of the year. Some discussion supporting this choice would be helpful. For instance, the warm versus cold season soil moisture differences in Figure 2 are (surprisingly) quite small. On the face of it, this lack of seasonality probably supports the author’s decision to consider seasonality in a relatively simple way.

2) Page 8/Lines 4-7 – The attribution of this “Southern California” signal to an irrigation effects is problematic. The area fraction of Southern California that is irrigated is actually quite low. It is much more likely that the lack of (VIC/SMAP) correlation in these areas is due to thermal problems with 6 pm retrievals over arid/semi-arid regions (which is why the problem does not re-occur in Nebraska) during the summer (basically, sum-





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mertime pm conditions violate the soil/canopy isothermal assumption that SMAP uses to retrieve soil surface moisture). One way to test this, would be to re-generate Figure 4a using only 6 am retrievals and see if the effect goes away.

3) Bottom of page 8…what exactly is meant by “raw” SMAP retrievals? Also, the list here seems to contain 4 comparisons not 6 (as stated in the text). Finally, the exact link between these 4 (or 6) comparisons and plotted results in Figure 3 is a bit unclear. A couple more explanatory sentences would help here.

4) Bottom of page 13/of page 14. It is not clear to me how the SMAP L4 product could possibly detect the impact of groundwater extraction (using a land model which does not consider the impact of well pumping on saturated zone calculations and assimilation observations sensitive to only the top 5 cm of the soil column). Therefore, the attribution presented here seems potentially misguided. This discussion should be either strengthened or removed.

Minor notes:

1) The abstract spends too much time discussing SMAP background (in the first paragraph) and too little time defining the contribution of this particular manuscript (see major point above).

2) The SMAP product version names in the manuscript differ from the “official” product names/acronyms (see https://smap.jpl.nasa.gov/data/)…good to use the official versions.

3) Page 3/Line 20…double parentheses.

4) Page 7/Line 4…better to say “too tightly bounded”.

5) Page 7/Lines 9-11…reword to clarify…unclear how the moment matching approach applied here differences from that of Sheffield et al. (2004).

6) Figure 5 needs a color key…not clear what grey shading indicates.





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7) Bottom of page 12. . .where exactly is this “grid analysis” presented? Unclear what is being referred to here.

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