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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Exploring seasonal and regional relationships between the Evaporative Stress Index and surface weather and soil moisture anomalies across the United States" *by* J. A. Otkin et al.

Anonymous Referee #1

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This manuscript uses a correlation analysis to look at relationships between the Evaporative Stress Index (ESI) and a number of variables the effect evapotranspiration (ET) and resultant ESI values. Different composting time periods were used (2-, 4-, and 8-weeks). The main results find that soil moisture and dew point depression more strongly related to ESI than temperature and precipitation. The authors state this could be potentially useful in forecasting changes in ESI and vegetative stress.

This is a nice study overall with a straightforward and easy to interpret analysis. These results will be of interest to the drought and agricultural community for detecting early

signs of flash droughts. The main issue with the paper is that there are several key references missing. Many times throughout the paper it is stated that changes in evaporative demand (E0) drive changes in ET and ESI yet E0 is never used in the analysis. The Evaporative Demand Drought Index (EDDI) is also never used or cited, yet this analysis essentially repeats the analysis from the EDDI paper. References about dominant drivers of E0 are also needed here. With a more thorough literature review and acknowledgement of prior analysis that find similar conclusions, along with some other modifications this paper could be acceptable for publication.

Comments:

1. My biggest concern is failure to acknowledge EDDI in this study. In particular, EDDI part II (McEvoy et al. 2016). This analysis essentially repeats that analysis and finds many of the same conclusions without reference to the paper. Figures 1 and 2 are basically the same as Figures 1 and 4 in McEvoy et al. (2016). Figure 4 in McEvoy et al. (2016) actually directly compares ESI with E0 (EDDI). Figures 4-6 are also very similar to Figure 3 in McEvoy et al. (2016) regarding the regional analysis and many of the same conclusions are found regarding poor correlations in the northeast. The main reason this is all so concerning is that the authors state many times that E0 is driving ET and ESI yet fail to recognize recent literature that has already show very similar results. This paper should be framed as building upon other work that has done similar exercises. With that said, the results in this manuscript do add nice additional insight around the individual monthly correlations rather than the seasonal means used in McEvoy et al. (2016), and around some of the individual drivers of E0.

McEvoy, D. J., Huntington, J. L., Hobbins, M. T., Wood, A., Morton, C., Anderson, M., & Hain, C. (2016). The evaporative demand drought index. Part II: CONUS-wide assessment against common drought indicators. Journal of Hydrometeorology, 17(6), 1763-1779.

2. The other key reference missing is regarding sensitivity of E0 to the individual drivers

over CONUS. Hobbins (2016) describes this nicely. The dominant drivers of E0 are different depending on region and season. And, a key point of Hobbins (2016) finds that temperature is not the dominant driver for many regions. This is likely having a big influence on the how ESI (with E0 as one part of the ratio) is correlating to the different drivers. There should be some discussion around this reference.

Hobbins, M. T. (2016). The variability of ASCE standardized reference evapotranspiration: A rigorous, CONUS-wide decomposition and attribution. Transactions of the ASABE, 59(2), 561-576.

3. How was CFSR chosen as the data set to be used in the analysis? Were any other data sets tested? I would think, at least for temperature and humidity, that other finer scale station-based data sets would be better. My first thoughts for other choices would be PRISM or gridMET (http://www.climatologylab.org/gridmet.html). For wind and solar a regridded CFSR would probably be just fine. Can you provide any justification (in the manuscript or response to review) for choice of data? 4. The standardization of different variables appears to have all been done using different periods of record. SPI: 1948-2015, ESI: 2001-2015, NLDAS: 1979-2015, CFSR: 1979-2015. I would think using the 2001-2015 period for all variables (standardization, not just comparisons) would provide the most robust comparison. Levels of wet and dryness will not be comparable using different time periods. Can you justify the reasoning for using different periods of record? 5. How were the regions in Figure 3 defined? Seems very arbitrary. Something like the NCEI climate regions would probably be better. Please provide more detail on how/why these specific regions were used. 6. The method for the correlation analysis is a bit unclear. Please add a summary of 1-2 sentences about the sample size for the time series at each grid point. This is the part that is not clear to me. For a given grid point and month, would there be 15 years x 4 weekly values (n=60)?

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