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## ***Interactive comment on “Global trend analysis of the MODIS drought severity index” by P. I. Orvos et al.***

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The present manuscript presents a first-time analysis of recent trends in high spatial resolution remote sensing-based data of drought severity. Even though the main focus is on analyzing data rather than the instrumentation and data processing itself, the information provided is potentially helpful for assessing the potentials of practical applications of remote sensing-based drought indices in general and of the MODIS-based DSI in particular. In my opinion, this aspect by itself warrants publication of the presented material.

Geoscientific Instrumentation, Methods and Data Systems might not necessarily be the optimal forum for discussing the results of this work in detail from a process-based

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perspective, but its rather broad scope clearly includes comparable studies with a focus on the measurement, instrumentation, or data processing aspects. However, in this case I would expect that the authors discuss more systematically what specific type of information can be revealed using the considered data set, and what are its advantages and disadvantages in comparison with other existing drought indicators. Specifically, from the text I understand that the data are too short to infer detailed information on climatic processes and patterns, but their unique spatial resolution allow tracing the effects of various types of anthropogenic interference at different spatial scales.

With this general impression, I recommend further consideration of this manuscript beyond the discussion stage given the authors address the aforementioned general point. In addition, I have a couple of more specific issues that I would kindly ask the authors to further comment on in a possible revised version of work:

1. p.21: When introducing the DSI data set, please spend some words on what high/low index values mean in terms of drought conditions. Since DSI is no "classical" drought index, the corresponding information would be helpful to easier assess what the trends observed in this study actually mean (in fact, the information is given implicitly later in the manuscript, but the reader would benefit from some explicit statement in the beginning).
2. The authors state that the observed trends reveal decadal-scale climate variability rather than long-term global climate change (e.g., last paragraph of the introduction). I agree that 12 years of observations are insufficient to claim long-term trends, but the observed tendencies could still be due to either long-term trends or decadal-scale oscillations or a superposition of both. One way to further disentangle this might be allowing for non-linear trend models. I agree that this might be beyond the scope of the present work. However, being more precise in what could be the reasons for the observed linear slopes is definitely appreciated.
3. Regarding to the previous comment, it would be interesting to see how the global

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trend patterns (Fig. 4) compare with the outcomes of similar analyses applied to classical drought-related indices like PDSI, SPI or others. These indices are available, though on coarser spatial resolution, for the same period as DSI (and even far beyond) and might therefore be very helpful to assess the actual climatological implications of the results of the present study. To repeat the point already raised above, I feel that the practical use of DSI for climatological studies (i.e. the analysis of large-scale trend patterns) might be rather limited, whereas this index appears more suited for studying local effects due to human activity.

4. When presenting numerical values of trends, the authors use the unit "DSI / year". Since DSI is dimensionless, this does not seem to be appropriate, so just "per year" should be correct. However, given the authors' statement that numerical values of DSI at different grid points should not be compared quantitatively, I wonder what one can learn from comparing numeric trend values at all. This particularly refers to Fig. 3 and the discussion in the final paragraph of Section 2. Specifically, giving mean slopes for all drying and wetting trends as well as corresponding standard deviations (p. 23) seems to be quite pointless if values are mutually inconsistent by definition. I may have missed the point here, so some clarification would be helpful.
5. Finally, some aspect that I would recommend for further considerations is seasonality. From a climatological perspective, it appears highly relevant to study the seasonal pattern of trends. Even though the authors emphasize that their analysis does not provide trends in a long-term global change context, the results of such an exercise (possibly in combination with other related indices) would allow for a more detailed climatological interpretation. Note that also the brief review of existing findings on existing findings and possible mechanisms related to the observed regional trend patterns in South America and India almost exclusively refers to seasonal phenomena.

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## Minor comments

- p.20, l.20: "have been emerged" should read "have emerged"
- p.21, l.29: "paid for" should read "paid to"
- p.22, l.3: "geographically correlated" (what is this?) should probably read "geographically connected" (cf. p.23, ll.18-19)
- p.23, l.17: "indicate" should read "indicates"
- p.24, l.3: "has been started" should read "has started"

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