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Interactive comment on "Reliability of meteorological drought indices for predicting soil moisture droughts" *by* D. Halwatura et al.

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Note to the editor and authors: As part of an introductory course to the Master programme Earth & Environment at Wageningen University, students get the assignment to review a scientific paper. Since several years, students have been reviewing papers that are in open online discussion for HESS, and they have been asked to submit their reports to the discussion in order to help the review process. While these reports are written as official reviews, they were not requested for by the editor, and we leave it up to the editor and authors to use these reports to their advantage. While several students were asked to review the same paper, this was not done to provide the authors with much extra work. We hope that these reports will positively contribute to the scientific discussion and to the quality of papers published in HESS. This report was supervised by dr. Ryan Teuling.

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Discussion paper

The purpose of this research is to compare two meteorological drought indices, the Standard Precipitation Index (SPI) and the Reconnaissance Drought Index (RDI), to a physically based soil water model. The methodology consists of five steps. In the first step, sites are selected, in the second step, SPI and RDI are calculated over one, three and twelve month time periods. Soil water pressures are simulated with the model Hydrus-1D. In the third step, the drought indices are compared to the simulated values. PF values, calculated from simulated minimum soil water pressures for each month, averaged over a depth of 5 and 30 cm are correlated with the SPI and RDI values. In addition, the failure rate (FR) and false alarm rate (FAR) are calculated. In the last two steps, sensitivity of the model is determined and accuracy of the model in case of data uncertainty is assessed. From the results, it seems that SPI and RDI perform quite well, and can be used in case of limited data availability. The relatively high FR and FAR values might be due to simplicity of the indices, time scales and potential errors in the simulations.

As the authors state in the introduction of the manuscript, monitoring and predicting drought is of great importance, especially since the occurrence and severity of droughts is expected to increase due to a future increase in climate variability, affecting agricultural production and ecosystem functioning (IPCC, 2014). In addition, data availability is an issue in many areas. Thus, evaluating simpler drought indices is important and the results of this paper are useful for practical applications, such as regional drought monitoring and water resource management. Much research is done on evaluating drought indices, as is also clear from the manuscript and SPI is often evaluated as meteorological drought index for agricultural or hydrological drought (e.g. Kumar et al., 2016; Sheffield et al., 2014; Sims and Raman, 2002). Furthermore, SPI and RDI are compared by e.g. Khalili et al. (2011).; Banimahd and Khalili (2013) and Shokoohi and Morovati (2015). In my opinion, it is interesting that in this research not only SPI are RDI are evaluated (research question 1), but the authors also aim to assess the effect of data uncertainty on simulations (research question 2). In this way, the research provides important information for regions with limited data-availability. The research corresponds to the scope of the journal, as it addresses drought, the temporal, and to some extent spatial, characteristics of precipitation and soil water pressures, and serves water management. The structure of the manuscript is good and the writing is clear. Some minor comments on the structure are provided in the last section of this review. In my opinion, there are some major issues in the methodology that should be addressed or further explained by the authors before the manuscript is published. I will elaborate on this in the following paragraphs.

Specific comments

Comment 1: In the third step of the methodology, SPI and RDI values are correlated with pF-values. SPI is defined by McKee et al. (1993) as: "Standardized precipitation is simply the difference of precipitation from the mean for a specified time period divided by the standard deviation where the mean and standard deviation are determined from past records." A probability function is defined for a specific time period (over periods of e.g. one month, three months, twelve month) and e.g. a three-month accumulated precipitation is compared to average precipitation over that same period in a range of years. This is supported by e.g. Lyon et al. (2011), Kumar et al. (2016) and Guttman (1999). As a result, SPI indicates anomalies from a certain seasonal precipitation pattern in a specific climate, that occurs in a certain region. The pF values that are calculated from the minimum water pressures for each month and site and averaged over a depth of 5 and 30 cm, do include seasonal variation and climatic differences. Therefore, it is strange that SPI is directly correlated to these pF values. The difference in SPI and pF values will probably have an effect on the correlation. This expectation is supported by the fact that according to the climate index Rw/RsÂň in Table 1 (P15), there is a seasonal pattern in rainfall. I recommend to standardize the pF values, see e.g. Kumar et al. (2016), who make a standardized index for groundwater measurements or Sheffield et al. (2014) (see P4), who create a drought index based on a cumulative probability of soil moisture fitted to simulated values. To ease comparison, the data might be normalized (to a range of [0,1], e.g. Sims and Raman, 2002). If the authors want to correlate SPI and RDI directly to the pF values, as pF is a direct indication of plant stress, I think it would be good to state in the methodology why they want to do this, what the effect on correlation is and to give an indication how large the seasonality is, e.g. by a graph. Figure 4 (P20) shows pF values and SPI values for Bourke, but the time period is so big, that yearly seasonality is hard to subtract from this graph. In addition, if pF is not standardized, the 75th is not appropriate. In my opinion, it makes sense that for the SPI a certain threshold is defined (P5, lines 17-24), and that this is equal for all sites, as SPI describes anomalies not absolute values. Values of SPI usually get assigned a certain category of drought intensity (McKee et al., 1993). In the methodology, the percentile for pF values is also set at 75. pF values are physically based and are directly related to available soil moisture, plant stress etcetera. In a wet climate, pF values are generally lower than in a dry climate. So the 75th percentile represents a different range of pF values for the different sites and could in a wet climate include pF values that do not lead to plant stress, whereas in a dry climate, exclude values that do lead to plant stress. I recommend that, if the authors compare SPI and RDI to the unstandardized pF values, that a threshold is defined that is set at a certain pF value and is equal for the three sites. In that case, the values of FR and FAR are no longer equal.

Comment 2: When reading the manuscript, for several sections I found myself wondering why certain decision where made or whether there had been previous research on certain topics. I will address four sections of the manuscript where, in my opinion, further explanation and references are required.

First of all, I expect that SPI, and to a lesser extent RDI, have been compared to more complex models and drought indices before. In the discussion, Sims and Raman (2002), Khalili et al. (2011) and other relevant studies are referenced , but in the introduction, no prior research on this is named. It would be good to embed this research in previous research while stressing the novelty of this research. This might also strengthen the methodology, in case other researchers use similar approaches. In the second paragraph of the review, I named some prior research. However, there are many more studies.

Secondly, from the introduction and methodology it is unclear to me whether the model Hydrus-1D or a comparable model is commonly used for drought monitoring and prediction. If this is the case, it would be good to emphasize this, as it stresses the relevance of comparing SPI and RDI with this particular model and assessing the effect of data uncertainty on the model output. There are studies on effects of drought, e.g. Hartman et al. (2012) on the effect of a.o. climate change on the soil water balance in relation to tillage, and Rahman et al. (2015) on the effect of drought on salt accumulation, but I could not find research on the application of Hydrus for drought monitoring.

Thirdly, it is unclear to me if Hydrus-1D is accurate enough to simulate soil water pressures and thus a proper replacement of soil moisture measurements. The authors state that there might be errors in the simulated soil water data (P9, line 10; P10, lines 12-17), while on the other hand, they assume the simulated water pressures accurate enough for the further analysis (P4, lines 28-29). The best solution would be to validate the model for the sites with actual measurements. This has been done priory, for example by Sheffield et al., (2014), who compare simulated soil moisture values with field measurements, before the values are transformed to a drought index. Another option, would be to at least provide some clarity on the accuracy of the model and how large the effect of errors on the correlation with SPI and RDI is.

Finally, in section 2, methods (P3-7), some argumentation and references on specific steps of the methods would be beneficial. A reference could be given for the FR and FAR in step 3 (e.g. Wilks et al., 2011, p.264), argumentation and references for the method to assess the model sensitivity could be given (step 4, but also affecting step 5), especially since the authors give some more suggestions for uncertainty analysis in the discussion (P10, lines 19-21).

Comment 3: In the fifth step of the methodology, water pressures are simulated for perturbed parameter values. FR* and FAR* values are calculated and compared to FR and FAR. My issue with this step in the methodology, is that it is unclear to me if the perturbation of the parameters corresponds to data uncertainty that can be expected in reality. Therefore, I cannot be sure if the statement "If FR*>FR or FAR*>FAR, the assumed parameter uncertainty in the hydraulic model critically affects its relative ability to detect droughts, so that the simple drought index may be preferred over the more complex soil water model, even if FR or FAR is high." (P7, lines 3-5) is true. The results of this comparison are shown in Figure 6 (P22), and the difference between FR* and FR is high, but is hard to interpret as it is not defined how realistic the data uncertainty is and in what circumstances this will occur. It is obvious that if data is absent in an area and cannot be obtained, the simpler drought indices are preferred. But if data is available, but uncertain, when is the uncertainty so high that simulation modelling should not be commenced? I recommend that the authors give an indication of what data uncertainty can be expected and either give an indication how close the perturbed parameters in Appendix C are to reality or redo the calculations for more realistic data uncertainty.

Minor comments

P3, Step 1 of the methods: To me it would seem more logical to describe the selected sites, than to state that selection is part the methodology. It the selection is part of the methodology, I would expect more information on e.g. selection criteria. I recommend to describe the sites at the beginning of the methods instead of naming it step 1 in the methodology.

P3, Equation 1: It seems as if the non-exceedance probability is multiplied with the three-month average value, which is probably not the case (P4, line1: "FR is non-exceedance probability of the three-month average value"). If so, the equation should be adapted.

P5, lines 20-21: "At the 75th percentile all drought index values below zero were taken as drought events." This sentence is unclear to me.

P9, lines 18-20: "However, notwithstanding the limited scope of this paper, our results point to the simplest being the best. Similar results have been observed in North Carolina showing that SPI is more representative of soil moisture variation (Sims and Raman, 2002)". Sims and Raman (2002) compare only two indices, SPI and PDSI and compare for a specific goal: short term variation in soil moisture. The research does not provide enough support for the statement that the simplest is the best. I suggest that the sentence is rephrased, or that additional references are given to support the statement.

P10, lines 7-9: "Given the more rapid variations in soil water content experienced in the shallow soil, if drought indices are used as a planning tool for initial establishment of seeds, a shorter time scale than three months is likely to be preferred." If so, then it might be beneficial to show the results for the 1 month time period calculations.

P10, lines 22-24: "Overall, SPI performed better than RDI, illustrating that in general the inclusion of PET in RDI confounds the prediction of drought for these sites, although this result may be affected by PET estimation and Hydrus model errors as discussed above." The discussion on comparing SPI and RDI could be embedded in prior research, see e.g. Khalili et al. (2011) (referenced in the manuscript), Banimahd and Khalili (2013) and Shokoohi and Morovati (2015).

P11, lines 22-23: "The study reveals that a simple drought index, SPI, which uses only monthly precipitation as an input, may out-perform a more sophisticated index, RDI, over a range of soil types and climates." To me, it was not instantly clear that SPI and RDI would be compared to each other as opposed to the physically based model (P2, lines 26-30). In addition, the difference between performance of SPI and RDI is discussed by the authors: "... although this result may be affected by PET estimation and Hydrus model errors as discussed above." (P10, line 23) and "For arid Bourke, there was less benefit using SPI rather than RDI (Fig. 5), which we speculate is due to a stronger and more linear influence of PET on the soil moisture minima at that site (Khalili et al., 2011; Asadi Zarch et al., 2015)." (P10, lines 25-27) and is contradicted by previous research (e.g. Khalili et al., 2011). This sentence might be rephrased to: "In this study the simple drought index, SPI, which uses only monthly precipitation as an input, out-performed the more sophisticated index, RDI".

P18, Figure 2: To have some attention for the very small details, the rectangle in the smaller scaled picture (showing Australia) does not exactly correspond to the large scale map and the North arrow in the small figure (showing Australia) might be excluded, as it is probably clear that the orientation of both maps is equal.

P20, Figure 4: This figure is referenced in the manuscript in the following sentence "Drought index values were greatest (most negative) in arid Bourke (-2.94), similarly the pF values for both soil profiles were also highest in Bourke (pF5 = 4.47 and pF30 = 3.38) compared to other two sites (Fig. 4)." (P 7, lines 11-12) However, this is not clear from the figure, as only results for Bourke are shown and it is hard to assess the exact value from the figure. Therefore, I think this figure does not serve its purpose in regard to this sentence. The authors might add a sentence that states what is in the figure (e.g. long term pF30 and SPI values for Bourke).

P21, Figure 5: This figure is to some extent unclear to me. It shows the correlation for pF values and SPI, supplemented with R2 values. This is clear, and the distinction between SPI and RDI using colours is good. However, the graph in the middle is not well explained. For every site, two values are shown, which probably represent correlation with pF30 and pF5, but this is not explained.

Some grammatical errors: e.g. P1, line 21 "the model provide"; P5, line 20 "We choose the 75th percentile to represents"; P7, line 9 "… showed reliable all sited". There are more grammatical errors, but as I understood, the manuscript will be proofread before publishing.

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