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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."

In this manuscript SPI and RDI are compared to soil moisture droughts using a physically based soil water model (Hydrus-1D) for three different climate zones. From this physically based soil water model monthly minimal values are compared to 3-monthly SPI and RDI. To calculate the Failure Rate (FR) and False Alarm Rate (FAR), thresholds for all values are set to the 75th percentile. The uncertainty of the model is taken into account by comparing perturbed input values to the original input values. If the FR and FAR for the perturbed values are higher than the original ones the simple drought index preforms better than the model. The FR an FAR for SPI and RDT ranged from 19% to 68%. There are three options stated why FR and FAR were not lower than 19%. It is concluded that the SPI preforms better than both the RDI and the model. However, to give a physically meaningful threshold it is advised to use a model over a drought index.

When reading this manuscript, I noticed several positive points. This research is daring, since comparing a soil water model to drought indices is not done often. The necessity for drought research is very clearly described in the introduction. The research done in this manuscript is line with the cope of this journal. However, there are some major issues that mostly involve the comparison of the meteorological drought indices to the model. Firstly, comparing an absolute value to an anomaly is not correct. Also, the thresholds taken to calculate FR and FAR are dubious. Finally, critical choices that have been made are not elaborated enough. These three points will be further explained in the next paragraphs. I also doubt the novelty of comparing SPI and RDI; these two drought indices are compared to each other multiple other papers already. Because the changes proposed will have an extensive effect on the methodology and thereby the results and possibly the conclusion, I would recommend a major revision.

The following arguments are specified in order of importance. 1) In step 3 of the method section it is stated that the minimum soil water pressure of each month is compared to SPI and standardized RDI. In my opinion comparing these two is fundamentally flawed. SPI and RDI are anomalies of a mean while the pF is taken as an absolute value. An

absolute value of pF does not say anything about how dry the soil is in comparison to normality. As it is used now, wet seasons would never have a drought according to the pF values, while the meteorological drought indices can point out a drought if there is less precipitation and more evaporation than normal. Therefore, a direct comparison of these two would be skewed, leading to misinterpretations of the correlation. If there is earlier research done saying that comparing the minimum pF value of a certain month to 3 monthly SPI or RDI is correct and how to interpret this result, I would like to have this explained and referenced to in the paper. However, in my opinion the comparison would be better if the pF is also transformed to a standardized index. I would propose to do this in the same manner as the SPI stated in McKee et al (1993). In that way all indices can be better compared to each other and relations found can be more easily interpreted.

2) The choice to take the 75th percentile as a threshold for the SPI and standardized RDI made in step 3 of the methods seems to be chosen arbitrary. How it is written now, the main reason seems that the values of FR and FAR would be the same if the 75th percentile is chosen as threshold. The threshold to define a drought occurrence for SPI and standardized RDI values is by definition 0. Every value below 0 indicates a drought. The table on page 2 of McKee et al. (1993) gives clear definitions of what values of SPI indicate certain types of droughts. In the paper of Tsakiris and Vangelis (2005) it is said that that for the standardized RDI the same thresholds can be taken. Therefore, I would suggest to take the threshold of 0 or one of the other thresholds stated in McKee et al. (1993). If the pF is calculated to an index in the same manner as SPI, the threshold would even count for this index. When this is done research question 1 (page 2 line 27) can be rephrased to: "Is it sufficient to use a simple drought index such as the SPI or RDI?".

3) Overall the choices made in the method seem arbitrary. There is little to no elaboration as to why critical choices have been made. The major issue that is not elaborated enough is the choice of using SPI and RDI and the Hydrus-1D model. In my opinion the

manuscript would be much stronger if more drought indices were taken into account. Given that the data is already available, this is relatively easily done. Therefore, I am curious to know why only these two drought indices are used. In the reference made for the Hydrus-1D model (Simunek et al., 2008), several other models for calculating soil moisture have been given, so it does not make clear why this particular model is chosen. In general, the method step 2 is not worked out enough. The reference period for the calculation of the non-exceedance probability of the SPI is not given. The calibration and validation period for the Hydrus-1D model is not given. In addition, there are too little references used in the methods from step 3 on, making the fundamentals of the research weak.

There are also more minor arguments to address before publishing this manuscript. 1) Other parts, more minor parts, of the methodology were not well elaborated. E.g., page 3 line 26. Why show the three-month averages and not the one or twelve-month instead? Or page 4 line 18. Why use 5 and 30 cm depth? Or page 6 line 10. Why use a range of -50% to + 50% for the perturbation?

2) Page 2 line 27-28. Research question 1 does not seem specific enough. If I understand correctly, it is meant as: Is it sufficient to use a simple drought index such as the SPI and RDI to estimate soil moisture deficits, (. . .)? If this assumption is correct, the conclusion does not answer this question. The conclusion answers the question: When comparing SPI and RDI, which one is better at detecting a soil water deficit?

3) Page 4 line 25. Assumption iii in the method section 2.2 states that "free drainage lower boundary condition is an adequate approximation". I doubt that this is the same for the three different climatic zones.

4) Page 10 line 18-19. Are the results and thereby the conclusions still reliable after stating that the outcome may be due to the accuracy of the model?

5) Page 9 line 1-2. Where is the phrase "a physically based soil water model should be used in preference" based on in this manuscript? I could not find evidence for this

statement in this research.

6) Page 3 line 2. The relevance to ecosystem restoration applications does not come back later in the paper. Please either leave this sentence out or refer to the possible applications of this research in the discussion.

7) Page 5 line 20. The reference used (Arnold et al., 2014) is very specific for the germination of one species. Stretching this to all phases of plant life and all plant types seems not right. A reference to a broader paper would be better.

8) Page 9 line 19-20. The research of Sims and Raman (2002) differs to much from this research to compare. Comparing to Khalili et al. (2011), Pashiardis & Michaelides (2008), or Zarch et al. (2015) would be more appropriate.

9) Page 1 line 31. The reference to Vicente-Serrano et al. (2010) does not seem a good reference for this sentence. Referring to Zagar et al. (2011) would be more appropriate.

Other minor issues are listed here in order of occurrence in the manuscript. Page 1 Title. There is no prediction it this paper. Please rephrase the title. Page 1 line 21. Change "provide physically" to "provides physically". Page 4 line 18. Change "soil depth" in "soil depths" and give the values instead of "(see below)" Page 4 line 22. Strange place of reference. Page 5 line 13. Please write "FAR" in italic like "FR" in line 11. Page 6 line 6. Check equation for parentheses. Page 6 line 25. Check equation on font size. Page 7 line 2. Change "Appendix B1" in "Appendix B2". Page 7 lines 9-14. Better to provide this information also in a table. Page 8 line 11-12. Please rephrase. Page 10 line 3-7. This might be better in the methods. Page 10 line 24. What actually implies that PET is more important for the shallower soils? Page 11 line 31. Please rephrase "unlikely to more useful" to "unlikely to be more useful". Table 1 and 2, and Appendix A. These do not seem very necessary. Table 2. Please explain symbols in the caption. Table 2. Please check superscript in row 2 and 3. Table 2. What is "10" doing under Bourke row 2? Table 3. Please change "soil water for" to "soil

water pressure for" in the caption. Table 4. I do not think this information is relevant for this paper. Figure 1. Block 1. Why state "25 years (1988-2013) (3 sites)" while on the Bourke site the data is from 1971-1996. Figure 1. Block 2. Change "Step 1" in "Step 2". Figure 4. In the text (page 7 line 12) it was said that some form of comparison would be shown between the sites. However, only the Bourke site is shown. Figure 4. Suggestion: draws the lines of the 75%tile threshold in this figure. Figure 5. Elaborate more on the middle plot, which one is the 5 and which one is the 30 cm soil depth. Now this plot is not clear and is better to be taken out. Figure 6. Why not take also parameter n for the calculation of FR*? All appendices. These could all be part of the regular tables and figures.

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Discussion paper