Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-467-SC1, 2016

Hydrology and Earth System Sciences
Discussions

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

D. Heuvelink

danny.heuvelink@wur.nl

Received and published: 30 October 2016

Manuscript hess-2016-467 by Halwatura et al.: Reliability of meteorological drought indices for predicting soil moisture droughts

Reviewed by Danny Heuvelink

"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."

This paper evaluates how well the SPI and the RDI perform at estimating soil moisture droughts simulated by a physically based soil water model. The analysis of this paper is based on three sites in Eastern Australia. The soil water pressure was simulated using the Hydrus-1D model. The performance of the two drought indices was measured by calculating the correlation between the indices and the simulated monthly minimum soil water pressures. It was found that there was a significant correlation between the drought indices and the simulated monthly minimum soil water pressure. For most locations and all soil depths the FR and FAR were below 50 %, meaning that the indices are able to capture the occurrence of droughts quite well. SPI performed better in total than RDI in terms of FR and FAR. The uncertainty in the model is quite high, but the model approach produces physically meaningful values which are plant species specific.

The importance of the investigation subject addressed by this paper is quite high. Drought indices are used in a lot of researches, because of their simplicity and their use of easy obtainable measurements like rainfall. It also fits nicely within the scope of HESS, because the importance of understanding and monitoring droughts is very high, especially with changing climate it is more than ever necessary to be able to monitor droughts accurately. This paper also uses techniques and approaches that follow the scope of HESS, like modelling, mathematic applications and uncertainty analysis. Assessing the performance of commonly used drought indices is highly valuable, the interesting part of this paper is that it compares two different drought indices not only to each other, but also to a hydrological based model. This paper can thus potentially add more knowledge on drought indices and the use of a physically based hydrological model, improving the field of Hydrology. The novelty in this paper is especially the assessment of drought indices performance with the use of a hydrological model, therefore creating opportunities to test the performance of the drought indices without actual soil moisture observations.

However, there are major issues in this paper; the most important issue is the fact that the drought definition of the drought indices and the physically based model is different and therefore their comparison is not as straightforward as treated in this paper, see argument 1. This needs a lot of attention and a drastic change of methods. Also the reasoning behind choosing the 75th percentile as drought threshold is not explained well, the choice for the monthly minimum soil water pressure is not explained and the comparison of the results with other papers is too marginal, see arguments 3, 4 and 5. From the list of minor arguments it becomes clear that a lot of claims or choices made in this research are not explained properly. Also the writing of this paper has to be looked at critically, because of the many small mistakes and incorrect sentences. Based on all this, it becomes clear that this paper cannot be published in its current form.

Argument 1 different drought definitions for indices and the model

The drought indices calculate anomalies from the average condition based on monthly averages over all years in the dataset. This definition makes it that the SPI always addresses drought as a deviation from the average case (Tsakiris and Vangelis, 2005; McKee et al., 1993). For the drought based on the simulation by the Hydrus-1D model, the 75th percentile of the monthly minimal soil water pressure is used as a threshold. This threshold pressure is not based on seasonality but just on the total monthly minimum soil water pressures. So the drought indices are based on differences between the monthly values and the average monthly values, and the simulated minimum monthly soil water pressures are not based on seasonality. Therefore the comparison between these two is not straightforward to make, because they look at different things. The drought indices don't look at absolute droughts, but to relative droughts while the Hydrus-1D model outcomes used in this studies say something about the absolute

droughts.

To improve this part of the research a clear definition of drought has to be given that is used for as well defining drought periods by the drought indices and for the Hydrus 1-D model. Therefore the use of the drought indices or the definition of drought according to the minimum monthly soil moisture pressure has to be altered.

Santos et al. (2013) used the SPI to create maps of a site in Portugal with monthly precipitation that responses with a SPI value of -1.28, the threshold they chose for severe drought. The difference in precipitation between months is very large. Using the same methods as Santos et al., 2013 all drought indices values which are considered to be a drought can be transformed to the rainfall amount it corresponds to. That value is than comparable to the monthly minimum soil water pressure.

My advice is to use the methods Santos el al. (2013) used and transform the SPI and RDI values back into rainfall amounts and compare those with the monthly minimum soil water pressure. For the RDI this would mean to transform it into a net rainfall amount (precipitation minus evapotranspiration). This method would assure that the comparison made between the drought indices and the model would be based on the same definition of drought. This change is very drastically, but necessary, and would in fact change a big part of the methods and all the results and conclusions.

Argument 2 why only SPI and RDI?

There is no clear reasoning why only the SPI and RDI indices are used in the comparison. Why not use more than only these 2 indices. If more indices are used, with all different complexity, a lot more can be said about the performance of drought indices compared to the model. More statements can be made on the success of less complex indices compared to the more complex indices. Other indices that could for instance be included in the research are: standardized precipitation evapotranspiration index (SPEI) and Palmer drought severity index (PDSI). Using additional drought indices would lead to more solid conclusions and better understanding of the performance of drought indices compared to a physically based model.

Argument 3 why the 75th percentile threshold?

In the paper it is not mentioned why exactly the 75th percentile was chosen to represent the drought threshold. There should be a clear reasoning why a certain threshold was chosen. First of all is there a need to choose a non-physically based threshold. In the methods there is stated that for instance the wilting point does not necessarily coincide with the stress levels of plants. But this is based on one research, Arnold et al. (2014). This paper only researches this for 1 specific plant species. There is no evidence given for other species, so it cannot be said that a physically based threshold is not appropriate. So more research needs to be done on that. When the approach of choosing a percentile is researched and assumed correct, than a clear reasoning has to be given why a certain percentile is chosen. Following are different thresholds used by other papers on which a drought threshold can be chosen.

Agnew (2000) states that the drought threshold for the SPI should be based on the probability of occurrence. Therefore a moderate drought would already be at an SPI of -0.84, or transformed to a percentile the 60th percentile. Mckee et al. (1995) and Komescu (1999) place the threshold for moderate drought at SPI=-1.00 , transformed to percentile this is the 68th percentile.

Svoboda et al. (2002) define different drought classes, namely D0 (abnormally dry, 20–30% percentile), D1 (moderate drought, 10–20%), D2 (severe drought, 5–10%), D3 (extreme drought, 2–5%) and D4 (exceptional drought, <2%). Translated to the percentiles used in this study , it would be the 70th percentile for D0, the 80th for D1, the 90th for D2, the 95th for D3 and the 98th for D4. So depending from which drought severity onwards a drought should be defined, the percentile has to be chosen.

Concluding there are different choices that can be made to define the threshold from which onwards a drought is recognized. Therefore a clear choice of threshold value should be given and it has to be explained. When after proper research it is found

acceptable that a non-physically based threshold is appropriate, the easiest solution would be to keep the 75th percentile as a drought threshold, because then there would be no changes in the conclusions. But add some references like the ones above to highlight that this is an arbitrary choice and many researches use different thresholds.

Argument 4 why the minimum soil water pressure?

Why is the minimum soil water pressure used, no explanation is given on why this is used and this refers to the minimum of the month, but is it not better to take the average monthly soil water pressure? Suppose you are at the end of a drought, but the 1st soil water pressure of a month is still high, but is declining the rest of the month. Than your monthly minimum soil water pressure gives a very high value, even though the average of the month is way lower. The drought index values average over this month, so it is not fair to compare this value with the minimum soil water pressure over that month. Changing the monthly minimum soil water pressure to average soil water pressure causes the results of this research to be changed to some extent, but it is unclear for me how big this effect would be, because I have no insight in the dataset used.

Argument 5 comparison of results

Sims and Raman (2002) are researching the performance of PDSI and SPI in estimating the soil moisture. It turned out that for their study SPI was considered to perform better in estimating the soil moisture than PDSI. But there are a lot of differences between the study of Sims and Raman and this paper. Firstly the comparison in this paper is with RDI and not with PDSI like in Sims and Ramans' paper. Although both include evapotranspiration, PDSI also includes local water availability. Also the study performed by Sims and Raman uses real soil moisture measurements, therefore it is hard to compare the results with this paper, that uses modelled soil water pressure. Also the area for which the researches are performed are different. Sims and Ramans' study is performed for North Carolina and this papers' research for Eastern Australia. So concluding it is very hard to compare the results of the paper of Sims and Raman with this paper and more references should be given to make a proper statement whether different researches came with similar results.

An example of another research that investigated the performance of the RDI and SPI is Shokoohi & Morovati (2015). They compared the performance of the RDI and the SPI for the Lake Urmia basin in Iran. The conclusions of this paper are that in a IWRM framework RDI is favourable over SPI. The paper of Zarch et al. (2015) states that the RDI index is favourable over the SPI index on a global scale in assessing future droughts, because of the increasing temperature trend. Although these researches all use different methods and the assed areas are different, it highlights that the result found for which drought index performs better is strongly influenced by the location and methods used in the research. So more references, like the ones provided above, should be given to compare the results found with others.

Minor arguments

1 The introduction is well structured and sets up the reason to research well.

2 The references to the fact that there are little soil moisture observations need to be more recent, more recent references are for instance Smith et al. (2012) and Peisch et al. (2012).

3 Instead of figure 2 showing only the location of the three selected locations, it can be upgraded by also listing soil type in the figure, to make the reasoning behind the locations chosen more insightful.

4 The layout of the figures and tables is quite plain and the bold and especially italic values are not easily seen. Maybe adding some colour in the tables would make them easier understandable and more appealing. Also the Bold and italic values could be indicated with different colours.

5 In the conclusion the focus lies more on the difference between the SPI and RDI, while in the abstract only 1 sentence is dedicated to this. Therefore adding a few

sentences in the abstract about this difference would make the abstract more representable for the research.

6 In the methods section 2.1, the selected sites are listed, but there is no clear reasoning given why especially these sites are chosen. Please add more explanation on why these sites have been chosen.

7 In the methods section 2.2, the selected average periods are listed and it is said that only the three month period will be shown, for the sake of simplicity. But are there other reasons why to only show the three month period. Please add some more reasoning on why only the three month average is shown.

8 there are several assumptions listed for the soil water modelling, but there is no insight given in whether these assumptions are valid or reasonable. Please add a paragraph in which the validity and reasonability of the assumptions is discussed.

9 the reference to Arnold et al. (2014) is very specific for one seed species and its' response to drought conditions. I advise to also refer to a broader paper about this subject.

10 in methods section 2.4, it is unclear why the simulated soil water pressure is considered sensitive to uncertainty if S>1, adding a few lines that explains this would make this section easier to understand.

11 No explanation is given on why the Hydrus 1-D model is used and not another model, please add some more explanation on why exactly this model is used.

12 I like the fact that an uncertainty analysis on the performance of the Hydrus-1D model has been performed and the discussion of this uncertainty analysis.

13 Figure 1, with the diagram of the steps in the methodology, helps to capture the structure of the methods better, therefore I advise to refer more often to this figure in the methods, to help the reader capture the links between the methods.

14 In the conclusion it is said that a False Rate of 19-32 % is considered to be a satisfactory performance, but in the discussion it is not explained why this is satisfactory, no comparison of these values were made with other studies, so it is hard to say whether these values are good or not.

15 figure 5 does not show the pF 5 values in the correlation graphs, but is said in the caption that it is treated in the figure. This figure is also really hard to understand, because of the missing explanation of what is shown in the figure. Maybe it is wiser to show the R2 values in bar graphs or a table.

Minor issues

p.1, line 20: missing "the" before "drought"

p.2, line 11: the "," after "e.g." has to be removed

p.2, lines 14-16: a reference that confirms this is missing

p.2, line 27: "to" is missing after "questions"

p.3, line 1: "Thorough" has to be changed into "Through"

p.4, line 18: "see below" has to be changed in "see next page"

p.4, line 22: the reference seems out of place here

p.5, line 23: "the" is missing before "same" (twice)

p.5, line 23: "of" has to be changed into "for"

p.5, line 24: "equation" has to be changed into "equations"

p.6, line 15: "uncertainty" has to be changed into "uncertainties"

p.6, line 22: a "," is missing after "(appendix C)"

p.6, lines 24-25: the font size of the equations is not the same

p.7, line 21: this sentence is unclear, maybe "all sited" has to be changed into "for all sites"

p.7, lines 11-14: these lines are should be rewritten, because they are unclear now.

p.7, line 18: a "," is missing after "default values"

p.7, line 19: "equation" has to be changed into "equations"

p.7, line 21: a "," is missing after "42 %"

p.8, lines 1-2: this sentence has to be rephrased to get the point across that with the 75th percentile the FAR and FR are the same, now it this sentence indicates that the FR, or FAR, at each depth and location is equal.

p.8, line 3: "is" is missing after "site"

p.8, line 14: "drought index" has to be replaced with "SPI"

p.8, line 16: "more so" has to be replaced with "especially"

p.8, line 16: "the" is missing after "in"

p.8, line 17-18: "relatively well" should be replaces with "better" and after "than", "for" is missing

p.8, line 28: "at" is missing after "frequency"

p.9, line 4: "for an example" has to be replaced with "for example"

p.9, line 8: "we" is missing after "best"

p.9, line 21: "into to" has to be replaced with "they were used for"

p.9, line 15: "for an example" has to be replaced with "for example"

p.10, line 9: "rather well" has to be replaced with "better" and "for" is missing after "than"

p.10, line 17: "index" has to removed

p.10, lines 17-19: please rephrase this sentence, because it is unclear now

p.10, line 24: “this implies” refers to the previous sentence, but it doesn’t match with the information provided in this sentence

p.10, line 29: “in” has to be replaced with “,”

p.11, line 17: “in” has to be removed

p.11, lines: 25-26: this sentence is not clear, please rephrase it

p.11, lines: 29-30: this sentence is not clear, please rephrase it

p.11, line 31: “be” is missing before “more”

p.11, line 31: “be” is missing before “more”

p.11, line 32: “Potential advantages” has to be changed into “ A potential advantage”

p.16, Table 4: “be” is missing after “may”

p.21, Figure 5: in the caption it is said that the 5 and 30 cm soil depth RDI and SPI are shown, but only the 30 cm RDI and SPI is shown

p.22, line 5: “represent” has to be changed into “represents”

p.25, Table C: not everything is at the right place in the header

References

C. T. Agnew, Using the SPI to Identify Drought, 2000, Vol. 12, No. 1, Winter 1999–Spring 2000, University College London, London, United Kingdom

Arnold, S., Kailichova, Y., andBaumgartl, T., 2014. Germination of Acacia harpophylla (Brigalow) seeds in relation to soil water potential: implications for rehabilitation of a threatened ecosystem. PeerJ 2 e268.

McKee, T.B., Doesken N. J, andKleist John, 1993. The relationship of drought fre-

quency and duration to time scales, Proceedings of the 8th Conference on Applied Climatology. American Meteorological Society Boston, MA: Anaheim, California, pp. 179-183.

McKee, T. B.; N. J. Doesken; and J. Kleist. 1995. “Drought monitoring with multiple time scales.” Proceedings of the Ninth Conference on Applied Climatology; pp. 233–236. American Meteorological Society, Boston.

Peischl, S., Walker, J. P., Rüdiger, C., Ye, N., Kerr, Y. H., Kim, E., Bandara, R., and Allahmoradi, M.: The AACES field experiments: SMOS calibration and validation across the Murrumbidgee River catchment, Hydrology and Earth System Sciences, Discuss., 9, 2763-2795, doi:10.5194/hessd-9-2763-2012, 2012

J. F. Santos, M. M. Portela, M. Naghettini, J. P. Matos & A. T. Silva, Precipitation thresholds for drought recognition: a further use of the standardized precipitation index, SPI, 2013, WIT Transactions on Ecology and The Environment, Vol 172, doi:10.2495/RBM130011

Alireza Shokoohi & Reza Morovati, Basinwide Comparison of RDI and SPI Within an IWRM Framework Water Resour Manage (2015) 29, 2011–2026, DOI 10.1007/s11269-015-0925-y

Sims, A.P., and Raman, S., 2002. Adopting drought indices for estimating soil moisture: A North Carolina case study.,Geophysical Research Letters 29(8).

Šimůnek, J., van Genuchten, M.T., and Šejna, M., 2008. Development and Applications of the HYDRUS and STANMOD Software Packages and Related Codes. Vadose Zone Journal 7(2) 587-600.

Smith, A. B., J. P.Walker, A. W.Western, R. I.Young, K. M.Ellett, R. C.Pipunic, R. B.Grayson, L.Siriwardena, F. H. S.Chiew, and H.Richter (2012), The Murrumbidgee soil moisture monitoring network data set, Water Resour. Res.,48, W07701, doi:10.1029/2012WR011976.

Svoboda M, Lecomte D, Hayes M, Heim R, Gleason K, Angel J, Rippey B, Tinker R, Palecki M, Stooksbury D, Miskus D, Stephens S (2002) The drought monitor. Bull Am Meteorol Soc 83:1181–1190

Tsakiris, G., and Vangelis, H., 2005. Establishing a drought index incorporating evapotranspiration European water 9/10 3-11.

Mohammad Amin Asadi Zarch, Bellie Sivakumar, Ashish Sharma, Droughts in a warming climate: A global assessment of Standardized precipitation index (SPI) and Reconnaissance drought index (RDI), 2015, Volume 526, Pages 183–195

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-467, 2016.