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Discussions

Interactive comment on "The Probabilistic Drought Forecast Based on the Ensemble Technique Using the Korean Surface Water Supply Index" *by* Suk Hwan Jang

Anonymous Referee #1

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For Korea, the authors suggested a new drought index called KSWSI which contains hydrometeorological components (such as Local water supply) which are not considered in the previous drought indices (e.g. MSWSI). Moreover, the probability of hydrometeorological components were estimated from long tail distributions instead of the normal distribution. Although the ideas of this study could be interesting, the current analysis results cannot justify their proposed KSWSI. The authors mainly subjectively claim their KSWSI is better.

Here are three main concerns:

(1) Inadequately setting out the research problem

In the introduction, the authors reviewed previous work. However, they did not motivate their study well. Many arguments are very confusing and subjective. For example, in P3 Lns 5-7, I don't understand why "the dry flood season caused by global warning". I don't know why mitigating droughts, improving drought indices and various conditions are related (P3 Lns 9-11). In the introduction, I would like to know the criteria of good drought indices. It seems that the authors know that intensity and duration are important (P4 Ln 1). However, the authors did not look at them. I am not sure whether the authors looked at "drought forecast" (P4 Ln 9; P2 Ln 11) or just "probability quantification". They know that lead time is important for drought forecast (P4 Ln 15), but they did not look at their forecast performance based on different lead times. Using different literature, the authors should explain why they want to include different new components individually. Based on other works, they should put some efforts to explain why the selection of distributions for different components is important. They need to justify why they only look at the Average Hit Score (AHS) and the Half Brier Score(HBS). They also should try to explain why they would like to look at maximum entropy principles.

(2) Poor analysis

The authors did not verify their results (P2 Ln 8). They just subjectively claim their results. The authors have many subjective and tautological statements. For example, "the accuracy of the drought forecasts using KSWSIs were higher than those using previous SWSI, demonstrating that KSWSI is able to enhance the accuracy of drought forecasts" (P2 Lns 10-12). After reading the whole section 2, I still don't know how they select appropriate variables to be the components KSWSI for different subbasins (P2 Lns 4-6). The authors claim that they investigate "all available hydrometeorological components" (P7 Lns 14-15). It appears to be impossible. I also don't know how they select distributions for their hydrometeorological components. At the moment, the authors assume that the most influential hydrometeorological components (P8 Lns 8-9) for their index would not change over year. The authors need a sensitivity study to demonstrate that. For Figure 8, I may suggest that KSWSI is overestimating drought risk because its boxplots cannot contain the actual droughts. Based on Figure 8, I would say MSWI is better than KSWSI. The discussion in P14 Lns 11-25 is very problematic. The whole validation of KSWSI is very subjective and qualitive. Here are some examples: In P17 Lns 2-3, the authors suggested that the “drought was avoided due to the abundant water resources”. In P17 Lns 17-18, the authors just subjectively guessed that “it is more reasonable that hydrological droughts occurred because of the low precipitation and dam inflow.” In P18 Lns 21-26, the authors just subjectively selected their probability distributions. Moreover, I don’t understand why a larger standard deviation of KSWSI suggests that the selection of probability distribution is correct (P2 Lns 17-20). Authors’ concepts of accuracy seem to mean larger uncertainty (P21 Lns 14-21; P22 Lns 4-7).

(3) Poor organisation

The details of the Average Hit Score (AHS) and the Half Brier Score(HBS) (P13 Lns 17-18) should be in the method section. The details of the maximum entropy principle (P20 Lns 7-24) should be in the method section. The conclusion is somewhat badly written. In P22 Ln 15, the authors just use a vague term, “limitations”. The authors need to think how to present their data, arguments and results logically.

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