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Natural Hazards and Earth System Sciences
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 #2

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This study proposes a new hydrological index to monitor and forecast drought events in Korea by using an improving version of the Surface Water Supply Index. The authors first compare the behaviour of the indicators for past events and then the forecast scores. Because of a lag in the scientific robustness of the results, of mistakes and wrong methodologies to compare the indices and their forecasts that reduce the scientific significance of the results, and because most of the results are too speculative, I am afraid to reject the paper. For the following version, I also recommend to strongly reduce the size of the document by adding most of the graph and descriptions in supplementary materials.

Here is a non-exhaustive list of major comments:

Introduction: The authors provide a large list of indices used in the literature but not used here. Either the authors add these in this study or they have to simplify the introduction. Also I found the quality of the introduction quite low. The state of the arts and the ongoing questions that motivate this study are missing. I recommend to completely reconsider this section.

Also to justify the fact that their index is more adapted over Korea, the authors should take into account other well known and commonly used indices they describe in the introduction and make a fair and objective comparison.

Figure 1, location of this basin in Korea ? Need to zoom out, orography ?

p4 L9 "Drought forecast ..." Which drought ?

Table 1b not clear at all.

Figure 2 Not visible

Figure 3 put in supp. materials.

Section 2.3 and figure 4 : Is it monitoring or forecasting ? as I understood it is monitoring. Please clarify.

Figure 5 + p10 L1-14. This is too descriptive, also without the climatology of each index, it is useless. What are the distribution and time variability of each value. How the authors 'validate' these graphs. It is really too speculative for me. I did not find scientific significance in these results.

P11 L2 how the authors can conclude that one method is more accurate? Again, all this section is too descriptive and the 'comparison' is done without scientific objectivity. That is why I consider all the conclusions too speculative without scientific evidences.

P11 L11 L14 what are the "scenarios" the authors mention? are there hindcasts?

Please clarify

Table 4 and its description (P12 L8) should be put in Supp. materials.

In section 3.2 why the forecast validation do not include 2001?

All this section of verification is done without scientific objectivity. Do we have to compare Fig 7 with Fig. 4? But if the two indices are different, with different behaviour and climatology, how the authors can compare these forecasts? A better way to assess these forecasts is to compare their improvements related to the climatology of each index (skill scores). Also I found a large part of this section too descriptive and could be improved.

Section 4: I found all this section quite hard to read. The authors propose to assess the uncertainty analysis of the calculation procedure for the KSWSI but, to me, this is not done properly. First the period of study is too short, what is the robustness of these few years? I do not understand why the authors use only few stations. A better way to do this study is to work with a longer time serie and all the stations available to provide robust statistics. Also, I was lost in that section that is too descriptive and too long. I suggest to completely reconsider this section.

According to all these recommendations, the authors should change the conclusions provided.

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