

Initial soil moisture condition is one of the most important climatic variables affecting the rainfall-runoff processes. In this study, the author presented a methodology to provide initial soil moisture condition for hydrological model based on the ERA5 Land Surface Reanalysis data. The topic addressed in this study is important and falls within the scope of HESS. However, the experiments were poorly designed on the basis of very strong assumptions that need to be verified. The results presented cannot justify the usefulness of the proposed methodology. With this regard, I suggest to reject this manuscript. Below, my comments on this manuscript are provided.

Major Comments:

1. Did the model reach the equilibrium condition with only 1-year warm-up? The author should confirm it.
2. The author should state which parameters were selected for calibration. In addition, based on the description in Section 3.1 and Table 2, the Case 1-4 were calibrated independently, which indicates that the values of parameters used for each case should be different. If that's the case, the performance difference between each case might also be related to the difference in the values of parameters. How does the author consider this? A reasonable configuration for Case 2 would be keeping all other setup the same as Case 1 while initiate the model without warm-up.
3. The design of the methodology builds on three very strong assumptions that need to be justified: 1) line 170, "We consider its simulated variable are the most representative of the hydrological model". How? Based on observation data or model performance? In line 269, the author further stated that "there is no truth value in this study", which is confusing. 2) Line 190, "we assume that its concept is similar to the value of ERA5-Land soil depth (289cm) minus S_a ". How? Do the two parameters in the two models represent similar physical process? 3) Line 196, "suppose we could get relationship between on of the BTOP variables and ERA5-Land SM variables". This is a strong assumption. What is it based on?
4. The author should justify the applicability of LSTM for this study. As an advanced data driven method, large amounts of data are needed to use LSTM, which is not the case presented in this study.

5. Instead of only validating the model performance over the period of 2008-2011, which is used for validating the performance of the curve fitting and LSTM, I would suggest the authors further validate the performance of the technique over some independent period.

Minor Comments:

1. Title: “Initial Condition” is not accurate, in fact only “Initial Soil Moisture Condition” is discussed in this study
2. Section 2.1, the author should label the places (e.g., Ojiya, Chikuma River Basin, Nagano and Niigata) mentioned in Figure 1. Otherwise, it’s hard for the readers to figure out the locations of them. In addition, I would suggest the authors remove the non-related information such as “one of the best rice-producing areas”.
3. The manuscript needs revision for language and grammar.
4. In terms of the computational expense, how much the proposed technique is more efficient than the classical warm-up method?
5. Line 125 is confusing to me.
6. Line 144, reference is needed after “hydrological simulation”.
7. Line 146, “a better performance” in what?
8. Line 107 uses “①” while Line 186 uses “(1)”. Be consistent.
9. Page 10, what is the time interval of simulated discharge.
10. Section 4.1, please consider rephrase the second and third paragraphs, which are really confusing.