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Comment on hess-2021-541

Anonymous Referee #2

Referee comment on "Analysis of flash droughts in China using machine learning" by Linqi Zhang et al., Hydrol. Earth Syst. Sci. Discuss.,
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Overall, I consider this to be a worthwhile contribution to the rapidly expanding flash drought literature. The authors provide a new definition that can be compared to other proposed definitions and they examine association with a range of potential drought predictors. My two major comments are on the framing and the comparison between flash droughts and "slow droughts."

Major comments:

1. The methods applied in the study are, formally, supervised statistical learning algorithms. While one can debate what "AI" means, I think it's fair to assume that very few people think of linear regression, or even nonparametric statistical approaches like Random Forest, as AI. LTSM does sometimes get put in the AI basket, but it's no longer really a leading edge, advanced AI application. All that to say, I was surprised by the content of the manuscript after reading the title, and I suspect others may be as well. The paper simply does not provide an AI-oriented methodological advance, nor does it present results that are interesting because of novel application of relatively new methods. For this reason I recommend retitling and reframing the paper to focus on the flash drought findings, and removing the prominent use of the term AI in title, abstract, and throughout the paper. There are many published studies in many fields that compare performance of parametric and nonparametric methods for various applications, sometimes including NN as well, and at this point I really think that the difference in performance between those

methods is best presented as a comparison of statistical methods that is useful but not particularly innovative. Instead, I recommend that the authors focus on their actual flash drought results in the framing of the paper, as those results are quite interesting for the flash drought community.

2. I appreciate the section of the manuscript that compares the predictability of flash drought to conventional drought. But in making this distinction the authors implicitly assume that flash and slow droughts, as distinguished using the RI threshold employed in this paper, are meaningful and relatively homogeneous types of drought with respect to the predictor variables. Are the flash droughts and slow droughts in the inventory relatively homogeneous and separable with respect to these predictors, when evaluated using standard clustering or homogeneity tests? And is there evidence of the greater spread in meteorological predictors for slow drought relative to flash drought, as the authors suggest when explaining poorer performance in predicting slow droughts as a function of meteorology?

Other comments:

1. I have no issue with the authors using their own, new definition to define flash drought events in their inventory, but it would be useful to, at a minimum, see a discussion of how the choice of definition is expected to influence results. Ideally, a comparison of inventories generated using one or two other definitions would be included.

2. The authors use a combination of ERA5 and meteorological station data. Can they show or cite a study that shows how consistent ERA5 is with meteorological station data in China?