

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
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Comment on hess-2022-90

Anonymous Referee #2

Referee comment on "Evaluation of water flux predictive models developed using eddy-covariance observations and machine learning: a meta-analysis" by Haiyang Shi et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-90-RC2>, 2022

In this study, Shi et al., presented a meta-analysis of the performance of machine learning (ML) algorithms in the estimation of evapotranspiration. While this manuscript is interesting and within the scope of HESS, I have a few major concerns.

Most importantly, while this is a meta-analysis, the authors were comparing results from different publications, in which different data sets and sites may have been used. That being said, some of the results are not directly comparable. For example, Zeng et al. 2020, may have selected a few sites that are much more difficult to predict; and can not be compared with the results presented in another publication. Also, some sites may use in-situ estimates of LAI and VIs, while others use LANDSAT or even MODIS LAI and VIs. In order to make their results publishable, they need to find a way to harmonize the data sets used in all studies. Or, they need to justify that they have an inclusion criteria when selecting all publications (instead of just stating we searched on Scopus). In addition, I am not sure whether the number of models they chose can well support their comparison of so many features.

Also, the authors have another paper looking at similar topics (even with some similar pictures and texts) in discussion on Biogeoscience. As an example, in this paper:

Line 114-117: And in machine learning, in general, modeling with unbalanced data (with significant differences in the distribution between the training validation sets) may result in lower model accuracy.

And in the BG paper:

Line 91-94: Modeling with unbalanced data (where the difference between the

distribution of the training and validation sets is significant even if selected at random) may result in lower model accuracy.

The only differences between the two papers is that the BG paper focused on NEE, while this paper looked into ET. I am not sure whether it is acceptable to publish two somewhat similar papers in two different EGU journals.

At the same time, overall, the writing of the manuscript is good. But I do find it difficult to follow from time to time. For example, the authors used many abbreviations without defining them (EVI, GPP and NDVI), and some of them may not be very familiar with all the readers.

Minor comments:

Line 34: I suggest that the authors refrain from statements like this, precipitation and runoff are at least equally important;

Line 52: detailed?

Line 155: I still do not understand why RMSEs are not used.

L186: outperformed whom? I believe that similar issues can be found in other places of the manuscript.