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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Comparison of six different soft computing methods in modeling evaporation in different climates" *by* L. Wang et al.

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The paper estimates pan evaporation by statistical models calibrated at 8 stations in China and briefly discusses the choice of climatic variables to drive the models. The paper can potentially be an interesting contribution to the field, but in my opinion it requires a revision to make it more attractive to the readers. I concur with most of the comments expressed by the other two reviewers, so I will not list any specific issues. A few things worth stressing on my view are:

(1) Statistical models. MLPs are in many cases used by default as the "soft computing" technique to statistically approximate geophysical relationships, so it is not a surprise to me that they came as the winner. Other methods may compete with the MLPs, but in my experience they are one of the best compromises between implementation

complexity and capacity to approximate mappings. The problem is that the MLPs are also well known for over-fitting issues, and as there is no cross-correlation tests in the paper (i.e., calibration and error performance on different stations), we may wonder if over-fitting may be playing a role here (other methods may be more robust in this sense, but will be penalized by showing a worst performance on the validation dataset). This needs to be discussed.

(2) Climate drivers. It may have been better to start by looking at the linear correlation between the climate drivers and the Ep at the different stations, to rank the relative importance of the drivers in a simple way. That could have been used to justify why RH and WS are only tested as part of the final combination of drivers to the models, and perhaps be used to reduce the number of driver-combinations to be tested. In my experience, the mapping between drivers and geophysical parameter has to be very non-linear for the linear correlations to differ significantly from a "correlation" inferred by applying first a non-linear estimator.

(3) Applications. My reading of the paper is that I better use MLPs to statistically mode Ep, but perhaps not for all climates. OK, but not sure whether that is a clear message to pass. I any case, I would suggest to go a bit further and use the constructed database to provide a more general model that it is not restricted to a specific climate type. This could have been tested by investigating cross-correlations (i.e., how a model trained in a station performs at a different station), and/or by calibrating the model with a database containing data from all stations. It is quite likely that the more general model cannot outperform the individual-station best model, but if the differences are reasonable that single-model could potentially be used to generate Ep over most China when driven with remote sensing data. To me, that would be an excellent outcome of the paper and of more utility than just showing that at a specific station one statistical model performs better than the others.

(4) Format. The paper, even with its current contents, needs a better way to present the results. There are currently more than 3000 numbers scattered around 12 tables

and around 70 scatter plots, with only one table ranking the models summarizing the main results. That material could be part of an appendix, but figures and/or tables synthesizing the main results are needed.

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