

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

**L. Wang et al.**

wang@cug.edu.cn

Received and published: 8 August 2016

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

C1

complexity and capacity to approximate mappings. The problem is that the MLPs are also well known for overfitting 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 overfitting 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.

Reply: we further evaluated the applied models with full weather inputs by changing training and testing period. Please see Table 14 for the results. A Discussion section was added into the revised version.

(2) Climate drivers. It may have been better to start by looking at the linear correlation between the climate drivers and the  $E_p$  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.

Reply: we have also added two input combinations as iv) RH and v) Ws. Now, the effect of each variable on  $E_p$  can be clearly seen. The statement “In overall, soft computing models with full weather data ( $R_g$ ,  $T_a$ ,  $H_s$ , RH and Ws) generally had the best accuracy. This indicates that all these variables are required for better  $E_p$  estimation. It can be seen from the applications that adding RH or Ws inputs into the applied models generally increase their accuracies in predicting  $E_p$  in all stations even though these parameters have the lowest correlation with  $E_p$  (see Table 1). This indicates the non-linear relationship between RH (Ws) and  $E_p$  and linear  $R^2$  indicator cannot show this phenomenon. Two-input ( $R_g$  and  $T_a$ ) soft computing models generally provide better accuracy than the SS model in predicting  $E_p$  in all stations in the testing stage. These simple models can be preferred in some areas (e.g, developing countries) where ob-

C2

taining the data of the other parameters (RH and Ws) are difficult.” was added at the end of the Discussion section.

(3) Applications. My reading of the paper is that I better use MLPs to statistically model  $E_p$ , but perhaps not for all climates. OK, but not sure whether that is a clear message to pass. In 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  $E_p$  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.

Reply: the best generalized model (MLP9) was also tested at each site. A Discussion section was added into the revised version.

(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.

Reply: all tables and figures were changed. We agree with you that there are lots of tables and figures. To the authors, however, all the tables and figures indicate main results. We could not select any table or figure for appendix.

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-247, 2016.