Comment on hess-2021-590

Anonymous Referee #5

Referee comment on "Pan evaporation is increased by submerged macrophytes" by Brigitta Simon-Gáspár et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-590-RC5, 2022

In this manuscript, the authors use three methods to calculate daily evaporation during summer months (Jun to Sep) at a experimental site in Hungary. The three methods are the FAO Penman Monteith (PM) method, multiple stepwise regression and Kohonen self-organizing (SOM) maps. All methods are provided with observed meteorological time series for the years 2015 to 2020 as input. Simulated values are then compared to the observed evaporation estimates of three evaporation pans. The difference between the three pans is that one is a standard open water pan, one pan is partly filled with sedimentes and one with water living plants. Overall, the Kohonen self-organizing maps yield by far the closest match to observed evaporation estimates for all three pans.

The topic of evaporation estimates is fundamental to land-surface hydrology. For example, the PM method is frequently used to estimated potential evaporatin in hydrologic models. Errors in the PM methods will bias any subsequent hydrologic modeling. Despite the fact, that the topic of the manuscript is of general importance, the structure of the manuscript is poor and needs to increase substantially. One important issue is that I could not find that all findings listed in the abstract and the conclusions mentioned in the abstract are supported by the main text. Additionally, the methods part does not provide all details to follow what the authors did exactly. This does concern the PM method and the SOM method. I am convinced that the comparison of evaporation estimation methods for the three pans (in particular Figure 6) are valuable findings that should be reported, but the manuscript needs to be substantially improved.

Most importantly, the authors should decide what their main focus of the study is. Is it the effect of macrophytes on evaporation or is it the different estimation methods? At the moment, this is not clear. I would suggest the former to be the more interesting subject.

Abstract:

L. 14ff: I don't think that the statement regarding the correlation of RH is supported by the findings of this study. See my comment below regarding L. 218ff.

The conclusion mentioned in the abstract on line 19f is not given anywhere else in the manuscript. It is unclear how the authors come to this conclusion or what they mean with "potential".

Introduction:

Section starting at line 52 is a collection of statements that do not follow a apparent logical structure. It is not clear to me what the authors wish to express here.

Methods:

L. 110f: The data described in Section 2.1 is not sufficient to apply the Penman-Monteith equation. Section 2.1 states that global radiation R_s is measured but Penman-Monteith equation requires net radiation and ground heat flux. How are the latter two derived?

Results:

L. 187f: There are only four lines describing the results of table 2. This is not well balanced. Either the text needs to be expanded or the table shortened.

L. 206f: I am not an expert in self organizing maps. I don't know how to interpret characteristics shown in table 3 and the authors only describe the last two lines in this table.

L. 218ff: "Thus, the correlation..." This sentence is confusing to me. First, it should state observed values and not modeled values. Second, it is shown in table 2 that RH is negatively correlated with E_p which is expected. Here, the authors state that red colors in Figure 5 show high correlation. For RH, the values are substantially higher than for any other variable suggesting a higher impact. This suggests to me that the SOM algorithm is not able to reproduce the relationships reported in table 2.

L. 231: I disagree with this statement. How can the authors state that all three methods are close to observed values, when coefficient of determination varies from 0.11 for Penman-Monteith method to 0.97 for self-organizing maps. I think it is fair to state that the Penman-Monteith method is not able to reproduce the observed values. It is not clear to me whether the authors did apply the Penman-Monteith equation correctly because not all details are provided in the manuscript (see comment above).

Discussion:

L. 256f: There are results reported in the discussion section. This should be moved to the results section and is not a clear manuscript structure.

L. 271ff: This section lists findings of other studies but does not provide a discussion of these results against the findings of the present study.

Minor comments:

L. 9: There is a misleading typo here: the A should not be capital.

L. 110f: Which equation was used to derive e_s and e_a from RH?

L. 118ff: Section 2.4 is not understandable to readers who are not familiar to SOM. It needs to be rewritten using an easier language. Figure 3 is also very confusing. Also, E_p is mentioned in Figure 3 as input variable, but this cannot be correct. I guess that observed E_p is used during training to compute an error measure.

L. 139: The sentence regarding the splitting of the data is incomplete.

L. 184: Table 1 can be moved to the appendix because it is not central to the goal of the manuscript.