Dear authors,

Thank you for your revisions in light of the initial review process. The manuscript and associated documents have been sent back out to the reviewers and they have identified changes and corrections to make before the manuscript is accepted for final publication. Before the manuscript is accepted for final publication, please could you address the comments below and show how these changes have been made throughout the manuscript.

Further, in light of the Reviewer comments, it is identified that the presentation of results should be clarified to ensure that this is clear to non-K-SOM experts. This could be aided by using more explicit definitions within the manuscript text. We added some new information related to K-SOM description. See them in the results section. The preceding discussions could be clarified to address wider related questions (e.g. how can standard Epot estimation procedure be modified to account for changes in the relation between measured air temperature and the temperature of the evaporating surface). The water temperature part was completed by the request of the reviewer. The water temperature was only used to discuss the evaporation difference between pan treatments. As the water temperature is not an easily available meteorological variable, it was not used in analysing the impacting meteorological elements on evaporation as a physical process. This might be a useful next step to take, so some discussion of this would be useful to the readership of HESS. But it was excluded in the aim of the study. The goal was to present the difference in evaporation between standard A pan and planted pans.

Kind regards,

Dr. Daniel Green

Guest Editor, HESS

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Reviewer 1 comments:

General comments:

I appreciate the authors' responses to my suggestions and their work on revising the paper. I still believe that some work on explaining the figures in more plain language would benefit the paper and increase readership. To this end, I would suggest that the authors' explain how to compare the different panels of K-SOM neighbourhood maps in figure 6. They say in their response to review that "These are not traditional Fig. with x and y axis. The Fig. 3 is a schematic layout of correlations, the winning nodes, and their neighbourhood." OK, that is fine, but if I am still confused as a reader, how can the authors improve the communication of the result? We added some extra information related to K-SOM when analysing the heat map, see section 3.2. Perhaps give an example to explain "winning" and "neighbourhood"? There is in section 2.4, first paragraph, lines 3-5. Can the authors explain how to compare the patterns in figure 6? Is it, for example, that if the blue in one hexagon in one figure corresponds to a red hexagon in another figure, then the variables are anti-correlated? A concrete example would be very useful, in my opinion. The following sentence was added to results: “When one variable is red, while the other one is blue on the same place of the heat map, the correlation between them will be negative.”

Reviewer 2 comments:

General comments:

The revisions have improved the paper, and it's good to see the addition of Figure 5, with the water temperature data compared at clear-sky and cloudy days. However, the data is not used optimally yet in the subsequent discussion. Standard Penman-Monteith equations use meteo-station air temperature, not because it is physically the most relevant parameter (one wants to be closer to the temperature of the surface at which evaporation happens), but rather because air temperatures are commonly measured and plant surface or soil surface temperatures are not. With the water surface temperatures in hand here, authors might propose physically based corrections to standard models (such as Penman-Monteith). As the SD of Ep data was very high in case of P-M method, see also R^2 values in Fig. 7, this assumption in Ep estimation is not recommended. Due to submerged macrophytes presence, we chose another standard model, the A pan planted with these crops. These plants are below the water surfaces and not above them. What was questionable in the study, was the increased water loss of evaporation pans due to submerged macrophyte present. This question was answered in the manuscript.

That the K-SOM fitting procedure can account for a much larger share of the day-to-day variability is interesting -- but not very useful for readers of the paper, as no resulting equations (or multi-step algorithms) are provided -- and even if they would, there is no way to know how relevant these would be at another location. If you could show that using the evaporating surface temperature, instead of air temperature, you could close the gap, that would be 'generic'. The relationship between estimated and observed Ep was presented in Fig. 7. The equations were also completed in the Fig. 7. Since the Ep of one sample place was included in the study, the ‘generic’ impact of submerged macrophytes on Ep was not fully discussed; maybe for different reasons, our results in other sites became variable. More surveys are needed to reveal the applicability of planted standard A pan Ep for different geographical and climatic conditions.

The robust findings that submerged aquatic plants can lead to increased evaporation at a water surface has some direct relevance in limnology; the physical explanation that these effects may act through modified temperature profiles is scientifically satisfying, but there might be a stronger answer to the 'so what?' question if specific adjustments to Penman-Monteith would be proposed. No due to submerged macrophytes below the water surface.

However, despite the preceding discussion, such expectation may not be feasible for the authors -- the minimum requirement for publishing this paper might well be that the basic data are made accessible to others who may be able to take the next steps that will have direct utility to the HESS readership. The data are available for request at the authors.

Details:
Title is not yet very attractive:

"Submerged waterplants increase pan evaporation"

"Pan evaporation is increased by submerged waterplants" Done

Specific changes:

Line 11 and 64 sipctatum ==> spicatum (as correctly used in line 110) Done

Reviewer 3 comments:

The authors provide a revised version of their manuscript taking my comments from the previous review into account. Most of my comments have been addressed, but some comments, especially on the visual interpretation of the K-SOM have not been sufficiently

answered. Additionally, the motivation to use the FAO56-PM is unclear. The motivation of the manuscript was to take into account the impact of the living organism embedded in the standard A pan, in tap water. It is well known that the potential evapotranspiration for plants (not inside the water bodies) can be calculated with the world-wide used P-M equation. In this study, the measured water loss of standard A pans with macrophytes below the water surface was analysed together with calculated potential evapotranspiration by P-M method. As these macrophytes are below the water surface, it was unknown which method may also be closer to the embedded pan evaporation. The accuracy of standard A pan based evaporation was assumed to be closer to the actual water loss of water bodies containing submerged macrophytes than the pot. evapotranspiration of P-M.

It is well known that several methods in evaporation estimation exist. We don't feel the necessity to complete our estimations (measurements and P-M method) with another one. The aim was to study the impact of submerged macrophytes on evaporation and not the calculation of lake evaporation. water loss I suggest the authors to add another method that has been developed explicitly to represent lake evaporation and not reference crop evaporation (see also comment below).

That would be another manuscript dealing with lake evaporation. The P-M determines the potential crop evapotranspiration and not lake evaporation. In case of empty A pans, the evaporation is assumed to be potential. We tried to answer the question – and not more - that how the sediment and submerged macrophytes might change the standard A pan Ep. Additionally, the use of language in the manuscript is poor with frequent incomplete sentences or poorly structured sentences that make the manuscript hard to understand. I provide examples of these below. Thank you, we corrected them. For these reasons, I recommend major revisions before the publication of the manuscript. I provide further comments below.

Additionally, the use of language in the manuscript is poor with frequent incomplete sentences or poorly structured sentences that make the manuscript hard to understand. I provide examples of these below. For these reasons, I recommend major revisions before the publication of the manuscript. I provide further comments below. The examples were taken into account.

Methods:

L. 144: "FAO56-PM may also be proper method to get pan evaporation with submerged macrophytes." This is an assumption that may not be true. As the manuscript states it may be proper and not more. And nobody revealed anything about water loss of submerged macrophytes until now. As the authors correctly state, it is a method to estimate crop evaporation, which is a very different application than lake evaporation. As the authors state, the equation they applied was developed for short reference crops (l. 151) which is not the setting here – yes, it is true but this is the standard formula to calculate reference crop evapotranspiration advised by the FAO and widely applied in wetlands (Allen et al. 1998; 2005). After all, this might explain why the PM method yields so poor results and might be simply not suitable to compare evaporation values for the experimental setups in this study. Previously, this fact has not been justified! We wanted to know which estimation is closer to water loss of those water bodies containing submerged macrophytes. Because evaporation physically means the loss of non-living water. There is no question in case of other plants above the water surface; their water loss is (evapo)transpiration and P-M use is accurate. But what is the happening with submerged plants? The FAO-56 method was the closest

method to standard A pan evaporation measurement, as the FAO also suggested the replacement of A pan evaporation by P-M.But not in this case. The authors should instead use a different method that is actually designed to represent lake evaporation. This was not the aim. Even, the effect of the submerged macrophytes is also missing from these formulas. Plenty of literature exists on this topic, see for example Harbeck et al. (1962) and also some others, based on Dalton formula. This new assumption should require a new manuscript with new aim, new processing and methodology. We should refrain from doing so. The authors should also stress that methods like FAO56-PM and Harbeck are special cases of the multiple stepwise regression methods. This latter sentence was taken into consideration (Methodology, 2.3 Section, first paragraph).

L. 183: Why do the authors stress that K-SOM is an indirect method? They state that it is indirect because it estimates evaporation from other quantities. The word “indirect” was emitted from the sentence. The same is true for the other methods so there is no reason to emphasize it for K-SOM. Although the standard A pan measures directly the evaporation. We refined the K-SOM definition in the methodology.

Results:

Figure 5: For the SM pan, the difference between surface temperature and 5 cm water temperature is higher between 8 to 16 hours than the difference between surface temperature and 15 cm water temperature. This is surprising to me and should be commented. The stratification of the water temperature in the manuscript is as described in the literature (Jacobs et al. ,1998). The only problem would have been the numbering of the layers (the closest layer to the pan bottom is the 5 cm, and all the others are above this height). The other numbers represent the height of the layer from the pan bottom. The sensor’s height was clarified in the title of Fig. 5. We clarified it.

L. 277ff.: The sentence starting with "The inputs..." is a listing of properties. It should be rephrased using simple language. We can’t simplify this description. The method requires this detailed explanation. The cited paragraph has been re-written.

L. 300ff.: To me, this paragraph, which describes Figure 6, is still confusing. I have raised this in the previous review already. The authors replied to my comment and I could follow that somehow, but the authors did not made an effort to provide a better description in the manuscript. For readers, who are not familiar with K-SOM, this paragraph will be hard to understand. really, we tried to improve the explanation of Fig. 6.

Language:

The language of the manuscript is poor. Sentences are often incomplete or use subjects that are not clear. This makes the manuscript hard to read and prevents publication. For example:

l. 82ff: "Lake evaporation..." There is an article missing in the beginning. The experimental site is not yet introduced because this sentence belongs to the introduction and so it is not possible for the reader to agree with this statement. The paragraph was re-phased. It belongs to the aim that is why it was placed here. The study site introduction is in M&M later.

l. 89: "region" is not introduced and thus, this sentence is not logic. It relates to climate. We corrected with referring to geographical position of Hungary and study site.

l. 90: "Months included in the study (from June to September)." This is not a correct sentence. It misses a verb. corrected

l. 192ff: The sentence starting with "The impact ..." is missing a verb and it is not clear what the authors mean. Thank you, done

l. 300: It should be SM treatment and not MS treatment. done

Minor comments:

l. 352: The conclusions from the previous study should be explicitly mentioned. corrected

l. 408: what is the "R" here, is it correlation or something else? Yes, completed

References:

Harbeck, G. E. J. (1962). A Practical Field Technique For Measuring Reservoir Evaporation Utilizing Mass-Transfer Theory. Geological Survey Professional Paper 272-E (Tech. Rep.). Washington It seems not the most actual work using the Dalton formula in estimating lake evaporation