Response to referee comment Anonymous Referee #2
We appreciate and would like to thank Anonymous Referee #2 for taking the time and effort to read our manuscript and expressing the generally positive impression of our work. We will use the constructive comments to improve our manuscript. Please, find below our point-to-point response (comment of the referee in **black**, our response in **blue**).

General comments :
The manuscript is well written and easy to read, although some sections need to be reworded throughout the paper to improve readability and highlight the scientific contribution of this study. My main concern is with the overall presentation of the article. I found that the innovative aspect of this study was hidden by general facts and conclusions that have already been proven in the past. I understand the importance of such conclusions in justifying a new parametrisation but, in my opinion, focusing on this key point weakens the overall quality of the article. I suggest that the authors improve the paper to focus on the importance of using the developed parametrisation and to emphasise the need for such a statistical model. In this spirit, I recommend to better integrate this work into the current scientific literature.

We would like to thank the referee for the well-supported specific comments. We think this has been of great help to us to make suggestions for changes. Through these suggested changes we think the manuscript will be more precise and focussed, and it will improve the overall quality of the manuscript.

Furthermore, some of the results discussed are not presented in detail and it is therefore impossible to review this information. You should either delete these results or provide the details.

We screened the manuscript regarding this issue, and in the specific comments you will find what we decided for each occurrence.

All the comments do not call into question the study itself. The material for a good article is already there and just needs some rearrangement and minor revisions. I am convinced that the article will gain in precision and interest with additional information.

We would like to thank the referee for this positive and supporting feedback. We appreciate that.

Specific comments are detailed below

Abstract

General comments : In this section, the objective of the paper is not clearly pointed out. In my opinion, you should focus on the specific parametrisation you proposed for Lake Ijssel based on the field measurements. All the elements are already written and you just need to rearrange the section.

We agree and we propose to change the abstract to:
"*We study the controls on open water evaporation of a large lowland reservoir in the Netherlands. To this end, we analyse the dynamics of open water evaporation at two locations, i.e. Stavoren and Trintelhaven, at the border of Lake IJssel (1100 km^2) where eddy covariance systems were installed during the summer seasons of 2019 and 2020. These measurements were used to develop data-*

driven models for both locations. Such a statistical model is a clean and simple approach that can provide a direct indication and insight of the most relevant input parameters involved in explaining the variance of open water evaporation, without making a prior assumptions on the process itself. This way, we find that a combination of wind speed and the vertical vapour pressure gradient can explain most of the variability of observed hourly open water evaporation. This is in agreement with Dalton's model which is a well-established model often used in oceanographic studies for calculating open water evaporation.
Validation of the data-driven models demonstrates that a simple model using only two variables yields satisfactory results at Stavoren, with R^2 values of 0.84 and 0.67 for hourly and daily data, respectively. However, the validation results for location Trintelhaven fall short (R^2 values of 0.65 and 0.44 for hourly and daily data, respectively). Using only routinely measured meteorological variables leads to adequate performing simple models at hourly (R^2 = 0.79 at Stavoren, and R^2 = 0.51 at Trintelhaven) and daily (R^2 = 0.86 at Stavoren, and R^2 = 0.83 at Trintelhaven) timescales. These results for the summer periods show that global radiation is not directly coupled to open water evaporation at the hourly or daily timescale, but it rather is a combination of wind speed and vertical gradient of vapour pressure. We would like to stress the importance of including the correct drivers of open water evaporation in the parametrization in hydrological models to adequately represent the role of evaporation in the surface-atmosphere coupling of inland water bodies.

-P1.L1 : I would talk about a « sink » rather than a « large loss term ». It is more adequate to the scientific level of the journal.

This sentence will be removed in the newly proposed abstract. But we agree with the feedback and we will adjust this in the introduction section P2.L3 to '*Evaporation is a sink in the water balance of inland water bodies.*'

-P1.L1 : « During summer seasons, which are projected to become warmer with more severe and prolonged periods of drought ». This is a general sentence whereas your study focused on a specific location. Even if we are on a global climate change path, the consequences (not specifically warmer summer) are not the same worldwide. You should be more specific on the spectrum of warming on the studied area (or region) and put references.

This sentence will be removed in the newly proposed abstract, but in the introduction section we will be more specific on the region that we are referring to, i.e. the Netherlands. In addition to the already mentioned references in the introduction targeted to mid-latitude regions in Europe, we will refer to the KNMI'14 climate scenario's for the Netherlands specifically.
We will adjust accordingly: P2.L7 – '*Summer seasons are projected to become warmer in the Netherlands, with more severe and prolonged periods of drought (Seneviratne et al., 2006, 2012; KNMI, 2015; Teuling, 2018; Christidis and Stott, 2021).*'

KNMI, 2015: KNMI'14-klimaatscenario's voor Nederland; Leidraad voor professionals in klimaatadaptatie, KNMI, De Bilt, 34 pp

- P1.L8 : « not available energy» Be specific on the type of energy.

With available energy we mean R_n – G (P9.L4), but we agree that in this context this may not have been clear. We will remove the term from the abstract as it does not support clarity of the sentence and the statement that we make there.

-P1.L11 : « main drivers » Be specific. What type of phenomenon they are the drivers of?

We agree we should be more specific here. However, in the proposed revised abstract this sentence has been removed.

- P1.L15 : « well performing simple data-driven models » I would be less enthusiastic with a R^2 of 0.51 and 0.43. The model is adequate but does not perform well.

We will add the required nuance of our statement. P1.L15 – '*Using only routinely measured meteorological variables leads to adequate performing simple models at hourly (R^2 = 0.79 at Stavoren, and R^2 = 0.51 at Trintelhaven) and daily (R^2 = 0.86 at Stavoren, and R^2 = 0.83[1] at Trintelhaven) timescales.'*

[1] Note that the numbers have changes after correcting a mistake (see our response to point 1 of anonymous reviewer #1)

Introduction

- P2.L1 : There is a more up-to-date review paper you should include: Woolway, R. I., Kraemer, B. M., Lenters, J. D., Merchant, C. J., O'Reilly, C. M., & Sharma, S. (2020). Global lake response to climate change. Nature Reviews Earth & Environment, 1(8), 388-403.
I would suggest to have a look at the paragraph about lake evaporation which gives essential materials for both your introduction and your discussion.

Thank you for the suggestion for this interesting paper. We will add the reference of Woolway et al. (2020) to P2.L2: '*Inland water bodies are known to interact with the local, regional and even global climate and are therefore highly sensitive to climate change (Adrian et al., 2009; Liu et al., 2009; Wang et al., 2018; Woolway et al., 2020).*

We will adjust and add the following to P2.L4-5: *'... how open water evaporation (E_{water}) will respond to these changing conditions. It is expected that changes in longwave radiation, Bowen ratio, ice cover, and stratification will affect the dynamics of E_{water} at the long-term (Wang et al., 2018; Woolway et al., 2020). Whereas at the shorter decadal timescale, a contribution to trends and variations in E_{water} is expected resulting from changes in wind speed, humidity, and also through global and regional solar dimming and brightening and its effect on water surface temperature (Desai et al., 2009; McVicar et al., 2012; Schmid and Köster, 2016; Wang et al., 2018; Woolway et al., 2020). During the summer season evaporation rates are...'*

Desai, A.R., Austin, J.A., Bennington, V. & McKinley, G.A. Stronger winds over a large lake in response to weakening air-to-lake temperature gradient. Nat. Geosci. 2, 855-858 (2009)
McVicar, T.R. et al. Global review and synthesis of trends in observed terrestrial near surface wind speeds: Implications for evaporation. J. Hydrol. 416–417, 182–205 (2012)
Schmid, M. & Köster, O. Excess warming of a Central European lake by solar brightening. Water Resour. Res. 52, 8103–8116 (2016)
Wang, W. et al. Global lake evaporation accelerated by changes in surface energy allocation in a warmer climate. Nat. Geosci. 11, 410–414 (2018)
Woolway, R. I., Kraemer, B. M., Lenters, J. D., Merchant, C. J., O'Reilly, C. M., & Sharma, S. (2020). Global lake response to climate change. Nature Reviews Earth & Environment, 1(8), 388-403.

We will add a sentence to P4.L4 (see specific comment on P4.L3): '*Adequate estimations of E_{water} are important in this context because there is a strong coupling between E_{water} and for instance lake level and extent, the lake ecosystem, and lake stratification and mixing regimes (Woolway et al., 2020; Jenny et al., 2020).*'

In addition, we will add the following to P18.L6 (see specific comment to P14.L22): '*This is similar to what has been found by studies of for instance Blanken et al. (2011) and McGloin et al. (2014), and it was noticed that intraseasonal variations of E_{water} can be linked to synoptic weather variations through these variables (Lenters et al., 2005, MacIntyre et al. 2009, Liu et al., 2011, Woolway et al. 2020).'*

- P2.L2 : « evaporation is a large loss term of water bodies ... » I would rephrase by saying this is « a sink in the lake water balance ». Also you should add a reference to justify this, even if it's a validated fact.

Thank you for the suggestion. We will change the terminology. We would like to argue that the fact that evaporation is a sink in the water balance of a lake can be referred to as common knowledge, and therefore does not need a reference.

- P2.L7 : « Summers are projected to become warmer » As mentioned for the abstract section, as you work on a specific location, you should be more specific on such fact as you are not working at global scale (mention the spatial scale). Moreover, I would recommend to give the climate reference on which the climatic comparison is made. You should also add a reference.

We agree that we should be more specific on the region. Therefore, we suggest to change the sentence to: '*Summer seasons are projected to become warmer in the Netherlands, with more severe and prolonged periods of drought (Seneviratne et al., 2006, 2012; KNMI, 2015; Teuling, 2018; Christidis and Stott, 2021).* This should make the statement more precise.

KNMI, 2015: KNMI'14-klimaatscenario's voor Nederland; Leidraad voor professionals in klimaatadaptatie, KNMI, De Bilt, 34 pp

- P2.L12-13 : « In terms of thermodynamics, ...» This works on some lakes but this is not always correct. In terms of thermodynamics, big lakes (such as the American or African Great Lakes) could be approach by using either 3D ocean model or 1D model. It will depends on the presence or not of the hypolimnion and furthermore on the stratification dynamic, if there is one.
See :
Xue, P., Pal, J. S., Ye, X., Lenters, J. D., Huang, C., & Chu, P. Y. (2017). Improving the simulation of large lakes in regional climate modeling: Two-way lake–atmosphere coupling with a 3D hydrodynamic model of the Great Lakes. Journal of Climate, 30(5), 1605-1627.
Gronewold, A. D., & Stow, C. A. (2014). Water loss from the Great Lakes. Science, 343(6175), 1084-1085.
Thiery, W. I. M., et al. "LakeMIP Kivu: evaluating the representation of a large, deep tropical lake by a set of onedimensional lake models." Tellus A: Dynamic Meteorology and Oceanography 66.1 (2014): 21390.

We agree that the statement we make here is not applicable to all inland water bodies and does depend on stratification and should be better focused on our target water body.

Therefore, we suggest to change it to: '*In terms of thermodynamics a shallow inland water body of only a few meters deep'*

- P2.L17 : This might be a detail but I would prefer to talk about a change of the amplitude (which suppose an increase of surface temperature during daytime but also a quicker decrease during night-time).

Yes, we are referring to the same process, using other words. We suggest to change the sentence to: P2.L16 '*…., where heat is stored in the lower atmosphere, vegetation and the upper soil layers. This leads to larger temperature amplitudes in sunny conditions, with strongly increasing surface temperatures and warming of the lower atmosphere during daytime, and strong decreases during night-time.*'

- P2.L21: Lake depth also controls the dynamical range of lake temperature amplitudes on diurnal timescale.

See also our response to point 3 made by anonymous reviewer #1 on the simulation of water temperature using a simple energy balance model of a water layer.
We would like to suggest to clarify the text at P2.L23 into: '*The subsurface energy budget implies that Lake depth controls the dynamical range of lake temperature amplitudes on diurnal timescale. Thus, instead of focussing at the surface only, rather the whole volume of the system should be considered.'*

- P3.L2-6 : This paragraph would gain in readability if you reduce the description to its essential. Penman equation is well-known and its description can be shortened. Moreover, this description is redundant with the one on P9.

We agree that the Penman equation is well-known, but we also think that it is important to stress the assumption made by Penman (i.e. assuming energy storage below the surface to be neglected, resulting in an instantaneous response of the surface temperature), which makes it less straightforward to use it for water bodies of a few meters deep. Moreover, it specifies the essential difference between the Penman and the Dalton equation. Overall, we think the balance in the introduction is good (i.e. 1 paragraph dedicated to the description of the Penman and the Dalton model). Therefore, we prefer to keep the text as is.

- P3.L7 : « Most studies ... » Is this sentence linked with the reference list starting on L9 ? If so, you should move the sentences « However, measurements of... » and « This can partly ... » elsewhere. Also, you said the contrary on L.26 "In the past, a number of studies reported ..." Moreover, I would not be that direct by saying measurements of evaporation from inland water bodies are under-represented, numerous studies have been published on the subject for the past 10 years:

Potes, M., Salgado, R., Costa, M. J., Morais, M., Bortoli, D., Kostadinov, I., & Mammarella, I. (2017). Lake–atmosphere interactions at Alqueva reservoir: a case study in the summer of 2014. Tellus A: Dynamic Meteorology and Oceanography, 69(1), 1272787.
Pillco Zolá, R., Bengtsson, L., Berndtsson, R., Martí-Cardona, B., Satgé, F., Timouk, F., ... & Pasapera, J. (2019). Modelling Lake Titicaca's daily and monthly evaporation. Hydrology and Earth System Sciences, 23(2), 657-668.
Moigne, P. L., Legain, D., Lagarde, F., Potes, M., Tzanos, D., Moulin, E. R. I. C., ... & Costa, M. J. (2013). Evaluation of the lake model FLake over a coastal lagoon during the THAUMEX field campaign. Tellus A: Dynamic Meteorology and Oceanography, 65(1), 20951.

Blanken, P. D., Spence, C., Hedstrom, N., and Lenters, J. D.: Evaporation from Lake Superior: 1. Physical Controls and Processes, Journal of Great Lakes Research, 37, 707–716, https://doi.org/10.1016/j.jglr.2011.08.009, 2011

Thank you for bringing up these references, we will include these in the study to integrate our work more into the current scientific literature.
Assuming that there is referred to the reference list at L12, it is not correct that these belong to our statement in L7 '*Most studies...*'. We therefore prefer to keep that as is.
However, we agree that our statements in L7 and L26 could read as *contradictory*, but what we mean here is that comparably there have been a lot more studies focussing on the understanding of terrestrial evaporation, and much less studies focussed on open water evaporation. Fortunately, there have definitely been studies that measured and modelled open water evaporation (also shown by the references provided by you). To avoid confusion, we suggest to change the sentence (P3.L7/8) to: '*However, comparably significantly less studies performed measurements of* E_{water} *from inland water bodies*.'

- P3.L27 : I'm not convinced about the utility of the brackets. Moreover you could also include other important hydroclimate variables.
Zhou, W., Wang, L., Li, D., & Leung, L. R. (2021). Spatial pattern of lake evaporation increases under global warming linked to regional hydroclimate change. Communications Earth & Environment, 2(1), 1-10

Agreed on the brackets; we will remove them.
The coupling between lake evaporation and hydroclimate (P-E) as referred to in Zhou et al. (2021), is found on larger timescales (decades), and a link is made to changes in lake evaporation under different climate scenarios. It therefore provides another concept to describe lake evaporation, but at another timescale than the other studies that we are referring to in these lines (P3.L29-31). We suggest to add the following (P3.L31): *'At larger timescales a spatial coupling was found between* E_{water} *and P-*$E_{terrestrial}$ *(Zhou et al., 2021). Jansen and Teuling (2020) studied the performance of a number of concepts that are commonly used to describe open water evaporation.'*.

- P3.L34 →P4.L2: Woolway et al 2018 & Wang et al 2018 have addressed this issue. Moreover, even if I agree with your assumption, I'm not convinced about such parametrisation for use at longer timescale (for example, seasonal timescale). Lake temperature and evaporation are interdependent on such timescales and other hydroclimate variables should also be included.

Woolway, R. Iestyn, et al. "Geographic and temporal variations in turbulent heat loss from lakes: A global analysis across 45 lakes." Limnology and Oceanography 63.6 (2018): 2436-2449.
Wang, Wei, et al. "Global lake evaporation accelerated by changes in surface energy allocation in a warmer climate." Nature Geoscience 11.6 (2018): 410-414.

Thank you for the additional references. Our statement about the disagreement of the methods on the average increasing historical trend of the evaporation rate, as well as for the projected future trends is based on the findings in our previous study (Jansen and Teuling, 2020). That study showed that the choice of method, with different representations of the evaporation process, can lead to significantly different projected trends. We do agree with the point you raise here that lake temperature and evaporation are interdependent on these longer timescales, which requires the water body energy balance to be represented correctly. That observation actually supports our statement that it is important to find a way to correctly represent the evaporation process for the timescale that is studied, i.e. hourly and daily.

We suggest to make the following adjustment P3.L1-2:
'At longer timescales (i.e. seasonal and yearly timescales) it is important to include the interdependency between lake temperature and evaporation. This requires a concept in which the water body energy balance to be represented adequately, for the correct modelling of the E_{water} process.'

- P4.L3 : This sentence is the key point of your study. More generally, the paragraph from L3 to L14 should be the core of your introduction. I would reduce the presentation of the different equation (Penman, Makkink) and enrich this paragraph.
Add a reference for this : « a crucial element in its water management system ». Also, you focus on the water management aspect however your paper does not specifically study the impact of the parametrisation on the lake hydrology. I would recommend to add other aspects of the evaporation as a component of the global energy and water cycle. For example, you can talk about the influence on the near-surface turbulence intensity, the stratification or the lake ecosystem.

Raymond, P. A., and others. 2013. Global carbon dioxide emissions from inland waters. Nature503:355–359. doi:10.1038/nature12760
Jenny, Jean-Philippe, et al. "Scientists' warning to humanity: rapid degradation of the world's large lakes." Journal of Great Lakes Research 46.4 (2020): 686-702.

It is correct that this last paragraph of our introduction includes the aim of our study. We brought it into context of previous studies and core concepts that have been used, and are still frequently used, in calculating evaporation (Penman, Makkink, Dalton). So we think it is important to introduce that as well in this section. We agree to add a reference to 'a crucial element in its water management system' (*Buitelaar et al., 2015*). In addition, this study was performed to bring the importance of the correct parametrization of E_{water} in context of lake hydrology. Especially because, as you mention, there is a strong connection between E_{water} and e.g. lake level and extent, lake ecosystem, and lake stratification and mixing regimes (Woolway et al., 2020). Through these connections, E_{water} affects the water management of the lake in terms of drinking water services and water availability for agricultural land (Jenny et al., 2020).
We would like to suggest to add the following text at P4.L4:
'....in its water management system (Buitelaar et al., 2015). Adequate estimations of E_{water} are important in this context because there is a strong coupling between E_{water} and for instance lake level and extent, the lake ecosystem, and lake stratification and mixing regimes (Woolway et al., 2020; Jenny et al., 2020).'

Woolway, R. I., Kraemer, B. M., Lenters, J. D., Merchant, C. J., O'Reilly, C. M., & Sharma, S. (2020). Global lake response to climate change. Nature Reviews Earth & Environment, 1(8), 388-403.
Jenny, et al. "Scientists' warning to humanity: rapid degradation of the world's large lakes." Journal of Great Lakes Research 46.4 (2020): 686-702.
Buitelaar, R., Kollen, J., Leerlooijer, C. (2015). Rapport Operationeel waterbeheer IJsselmeergebied - Inventarisatie huidige waterbeheer IJsselmeergebied door Rijkswaterstaat en Waterschappen. Report. 112 pp. Grontmij. Alkmaar.

- P4.L12-15 : In this sentence, you compare Makkink's equation with Flake simulations. I'm wondering why you are not using FLake directly for lake IJssel if you consider Flake simulations as your reference ?

We agree that in this sentence it seems that we treat Flake as a reference. However, the main focus of our paper is to develop a simple statistical model from measurements, which can provide a simple and clean solution. So we use the measurements as a reference. To our knowledge Flake was never tested before against EC measurements for Lake IJssel. The comparison is made because Flake is a physically-based model, which is also integrated in the ECMWF model for instance, and we therefore assume it will perform better than the Makkink equation at these timescales, while the latter is currently used for estimating evaporation from Lake IJssel, so that is where our interest lies.

Data, Material and Methods

- P6.L8: It seems that the KNMI station only measure global radiation (as I see in the data provided), however would it be possible to have access to the four components of the global radiation? As shown in Wang et al 2018, the incoming radiation has an effect even if it's at longer timescale.
Wang, Wei, et al. "Global lake evaporation accelerated by changes in surface energy allocation in a warmer climate." Nature Geoscience 11.6 (2018): 410-414.

The assertion is correct: the operational KNMI stations only provide global radiation data (i.e. incoming solar radiation). Thanks to your remark here however, we did notice we have not explained how we obtained net radiation from the meteorological data. This will be added to section 2.5, where the Penman equation is explained. At longer timescales global radiation indeed will affect evaporation rates, that is why we also included this variable in the regression analysis.

P9.L6: '*... from the water surface. Net longwave radiation was calculated according to the equations* $L_{in} = \varepsilon_a \sigma T_a^4$ *and* $L_{out} = L_{e,\,out} + (1 - \varepsilon_s)L_{in}$*, (equations 2.24 and 2.28 in Moene and Van Dam, 2014) and net shortwave radiation as* $K^* = (1-\alpha)K_{in}$ *(Allen et al., 1998) with average monthly albedo values calculated as function of latitude (Cogley, 1979).*'

Moene, A. F. and van Dam, J. C.: Transport in the Atmosphere-Vegetation-Soil Continuum, Cambridge University Press, Cambridge, 2014.
Allen, R. G., Pereira, L. S., Raes, D., and Smith, M.: Crop Evapotranspiration - Guidelines for Computing Crop Water Requirements - FAO Irrigation and Drainage Paper 56, United Nations-Food and Agricultural Organization, 1998.
Cogley, J. Graham. "The Albedo of Water as a Function of Latitude." *Monthly Weather Review* 107 (1979): 775-781.

- P6.L30: Could you please rephrase this sentence to improve readability.

We suggest to change the sentence into: 'Firstly, the raw data were quality-controlled using several criteria in order to remove faulty or corrupted data.'

- P7.L30-32: "A regression analysis …" + "To develop ..." Please rephrase to improve readability.

We suggest to change the sentence into: 'A regression analysis was performed to explore which variable, or combination of variables, can best explain the dynamics of E_{water}. Variables included in this analysis were wind speed, VPD, global radiation, vertical vapour pressure gradient, air temperature and water temperature. From the regression analysis a data-driven model was developed to estimate E_{water} of Lake IJssel. This was done for both locations, Stavoren and Trintelhaven.'

- P8.L11: I found difficult to understand the justification of using such regression model and how the hypothesis of such model have been tested.
What are the type of estimator you used (I assume an ordinary least square estimator)? Did you perform a significativity test? It would be interesting to look at the result of the multiple linear regression model and specifically the p_value to include or exclude predictors. Are the period chosen representative of the population?

The functional form of the regression models was chosen to be a simple combination (sum or product) of variables only considering linear regression, multiple linear regression and quadratic regression models, which was a data-driven decision rather than a process-based decision.
Yes, statistical significance was tested on the used models (p-values < 0.05; this will be added at P8.L10 '*... a single variable. Statistical significance ($p < 0.05$) was tested. From the multitude...*'), meaning the best and simple models that we continued working with: Fig 6+7 and Table 2. The Venn diagrams however show the model fit of all combinations, without an indication of significance. We will adjust this to the Venn diagrams by removing those values where the model fit was found insignificant. We will add to the caption of the Venn diagrams: '*Values were removed if the model fit was found to be insignificant ($p < 0.05$).*'
Since our aim is to study the surface-atmosphere coupling, we did not perform gap filling, resulting in less data points, but avoiding the use of 'artificial' data. That is the trade-off to be made and as a consequence the summer period chosen for calibration is all data that we have. The fact that the chosen models that were fit on this period are significant, provides us confidence, as well as the relatively good validation results.

- P8.L22: "surface temperature" Are you talking about the Meteosat product? Hence, why do not use directly these field data? Are there representative of the surface temperature (the lake is shallow then it would be important to be sure the measurements are not performed in the thermocline).

What we are trying to convey here is that there are no routine observations of the surface water temperature, or the skin temperature, measured with for instance thermal infrared cameras. Due to these lacking camera's we were curious to find out if water temperature measured routinely at 1.2 to 1.5 m deep would suffice in estimating E_{water}. To clarify this we suggest to replace the text at P8.L20-22 by: '*There are no routine observations available of the skin water temperature of the lake. As an alternative, the use of water temperature data routinely measured by Rijkswaterstaat at depths ranging from 1.2 to 1.5 m was explored*.'

Results

General comments: In this section, I would have a distinct paragraph presenting the results of the calibration, another for the validation and a final one for the result on the routinely measured variables. It would also improve the readability as I had hard time following this section.

Thank you for this feedback. In response to this valuable suggestion, what we will do to improve readability is to make this distinction in presenting the results of 3.4.1) the calibration, 3.4.2) the validation, and 3.4.3) the routinely measured variables, as subsections of Section 3.4.

- P11.L5; Fig2: How did you choose the time period presented in the figure? Why do you present this period instead of either the training or validation time period? As I understand your paper is about summer and here you present a part of the autumn season. I admit I was a bit lost. Also I would

suggest to be more specific on the time period (e.g: 01/05-31/08 instead of nouns (You write May-August most of the time and once May-September).

The idea behind this figure, as well as figure 3, was to explore how the dynamics and trends of the meteorological variables, and especially the heat fluxes, would evolve before, during and after the summer, to explore if any lags for instance would occur. That is the reason why we presented here the period 01/05 – 30/09.

To clarify this we suggest to add the following to P10.L2: '*This figures illustrates the dynamics and trends of the meteorological variables and the heat fluxes before, during and after the summer period to explore if any lags for instance would occur at this timescale.*'

And at P11.L2 '*The monthly average diurnal variability of observed LE, based on hourly data, are shown in the top panels of figure 3 for location Stavoren for the same period as in figure 2 (i.e. 01/05/2019 – 30/09/2019).'*

- Section 3.2: In this section, you compare meteorological conditions. It seems you are comparing the air temperature and the wind speed that are are measured at different height. Measurements at Stavoren are made around 7m and around 10m at Trintelhaven. Did you adjust your measurement to an equivalent height? If not, this could explain some of the discrepancies. I have the same question for EC data. Moreover, you compared these variables to the Dalton model which needs variables at 2m height. What was the procedure you used to adjust the measurements to this height?

You are right that we have not adjusted our measurements to an equivalent height. We will make a remark on this at P6.L6: '*The measurement height at the two locations Stavoren and Trintelhaven differ. In our analysis we have not adjusted the measurements to an equivalent height. In theory, the small height difference will not affect the heat fluxes under the assumption of a constant turbulent flux layer.*'
Furthermore, the difference in wind speed found between the two locations cannot be explained by the difference in measuring height, as the resulting average wind speed actually is higher in Stavoren which is measured closer to the surface. The regression coefficients found might differ slightly because of the difference in measurement height. However, we do think that the variables that were found to be most important to explain the variance of E_{water} (i.e. wind speed and vapour pressure gradient) will not change.
Considering the remark about comparison with the Dalton model – there are two notes on this: 1) in the caption of figure 3 ('*Note that some variables included in the evaporation models are measured at larger heights than the 2 m that are prescribed (see Eq. 6 – 9)*', and 2) in the discussion at P18.L12-14. This should be taken into account. However, we do think using variables measured at greater heights will not change the diurnal trends that were found.

- P10.L20: "the water temperature at" instead of "the water at".

P.10.L10: Correct, will be adjusted.

- P10.L18: Please provide the correlation score to justify this is a strong correlation.

We will indicate the correlation score here, which is R^2=0.61. But, moreover, what we would like to indicate here is that from looking at time series only, one could see that latent heat flux and wind

speed are showing similarity in their trend, suggesting a good correlation between the two variables. This gives an indication for further analysis. Therefore, we will change the sentence into: '*The latent heat flux displays similar trends as the measured wind speed, indicating that the two variables are correlated (R^2 = 0.61).*'

- P11. Fig2: Please put the graph corresponding to the 2020 summer period in Appendix.

Agreed. We will do this.

- P12.L1: I would use the word "pattern" instead of "rhythm".

Agreed. Will be changed.

- P12.L5: Could you please add the graph in the Appendix?

We will do that indeed.

- P13.L17: Could you please add the correlation score to justify if it's a strong correlation. Also, be careful not to mismatch between correlation and determination when you analyse your results and even more as you are studying a non-linear model. Please rephrase the sentence "Global radiation …" to account for this difference.

The square of the correlation coefficient between wind speed and latent heat flux following from figure 2 is R^2=0.61. To match this with previous wording we will change it into '… with the adequate correlation (R^2=0.61) visible in figure 2.'. Furthermore, we will adjust P13.L17 to 'Global radiation and VPD have the lowest adjusted coefficient of determination, which agrees with our findings in figures 2 and 3.'

- P14.L4-6: I understand that you only exclude the global radiation from your model based on the R^2, however, in my opinion a R^2(VPD)=0.05 questioned the inclusion of VPD in your model. For example, adding VPD on the Stavoren hourly analysis has a limited impact which is not significant in Trintelhaven. Does VPD has a significant impact on your score?

We have checked the addition of VPD to the model again, and indeed it appeared to be significant. However, due to a mistake that we found as a result of one of the feedback points from anonymous reviewer #1, the Venn diagrams changed: only slightly for the hourly diagrams, but quite significantly for the daily diagrams. As a result, the best regression model is the same as the simple regression model: LE_{mod} = 5.1 uΔe + 1.6 (uΔe)^2 + 42 (R^2 = 0.74). Adjustment of the figures and accompanying text will be changed accordingly.

- P14.L22: Would it be possible to have some basics statistics on these data (mean, standard deviation, quantiles, min, max). It would also help to see if outliers are ejected from the analysis. A Table placed in the Appendix would be sufficient and would give a hint about the discrepancies between both summer seasons.

We will add the following table to the Appendix:

(a) **Stavoren**	Mean	Stdev	min	max	Q25	Q75	*N*
Hourly							
2019	103.6	69.1	-24.4	516.1	56.8	132.3	935
2020	128.0	87.9	-27.7	444.6	66.1	164.5	716
Daily							
2019	117.0	63.9	51.3	296.3	71.7	139.1	25
2020	154.6	82.8	56.5	365.1	102.6	194.2	18

(b) **Trintelhaven**	Mean	Stdev	min	max	Q5	Q95	*N*
Hourly							
2019	93.9	59.2	11.0	333.9	46.2	128.9	473
2020	90.9	59.8	-34.8	351.8	51.6	112.9	697
Daily							
2019	109.8	49.4	35.2	213.8	74.1	140.8	12
2020	97.4	49.8	37.0	261.6	69.4	104.7	19

- P14.L15: R^2 explains 45% of the variance which is quite low. If you include the water temperature, it reaches 0.48, it is still low but better. You limit the maximal number of variables for the simple model but in this case it would benefit to your model to add the water temperature.

Due to solving the mistake that we mentioned earlier the numbers have changed as well as the combination of variables leading to the best and the simple model. The Venn diagram of Trintelhaven where the sum of variables is given for daily timescales (Fig. 5d) now gives the highest R^2 for wind speed and Δe. Additionally, it is simply a choice to explore both 1) the best model which can include as many variables as possible, and 2) to limit the simple model to two variables at the most, as explained in P8.L12. Adjustment of the figures and accompanying text will be changed accordingly.

- P14.L22: "This can be attributed ..." As noticed in Woolway et al 2021, lake evaporation is highly dependent on weather variability (through its dependence to the lake surface temperature). Your discussion need to stress this issue and not just focus on the comparison to a mean climate. I would remove this sentence and discuss about this point in the adequate section.

Thank you for this suggestion. We will add some words on this topic to the discussion section for completeness. However, we think that the dependence of lake evaporation to synoptic weather variations through the dependence with lake surface temperature is actually included in the regression analysis, where T_{water} and vapour pressure gradient (which combines wind speed and humidity, both also related to synoptic weather variations) are two of the variables that were included.

We will adjust the following to P18.L6: '*This is similar to what has been found by studies of for instance Blanken et al. (2011) and McGloin et al. (2014), and it was noticed that intraseasonal variations of E_{water} can be linked to synoptic weather variations through these variables (Lenters et al., 2005, MacIntyre et al. 2009, Liu et al., 2011, Woolway et al. 2020). The same ingredients of wind speed and vapour pressure gradient were used in the model by Dalton (1802).*

Lenters, J. D., Kratz, T. K., and Bowser, C. J.: Effects of Climate Variability on Lake Evaporation: Results from a Long-Term Energy Budget Study of Sparkling Lake, Northern Wisconsin (USA), Journal of Hydrology, 308, 168–195, https://doi.org/10.1016/j.jhydrol.2004.10.028, (2005)
MacIntyre, S., Fram, J.P., Kushner, P.J., Bettez, N.D., O'Brien, W.J., Hobbie, J.E. & Kling, G.W. Climate-related variations in mixing dynamics in an Alaskan arctic lake. Limnol. Oceanogr. 54(6, part 2), 2401-2417 (2009)
Liu, H., Blanken, P.D., Weidinger, T., Nordbo, A. & Vesala, T. Variability in cold front activities modulating cool-season evaporation from a southern inland water in the USA. Environ. Res. Lett. 6(024022) (2011)
Woolway, R. I., Kraemer, B. M., Lenters, J. D., Merchant, C. J., O'Reilly, C. M., & Sharma, S. (2020). Global lake response to climate change. Nature Reviews Earth & Environment, 1(8), 388-403.

- P14.L26: "this confirms ..." Rephrase this sentence. If the ingredients are the same than in the Dalton's model why do not use this model or use a calibrated version on your lake?

The aim of our study is to find the drivers of E_{water} based on observations, without predetermining the variables to be included. The observations were used to develop the regression models, which confirmed a very similar relation to what was found by Dalton. We think this only helps to gain confidence to the fact that indeed wind speed and Δe are the most important drivers of E_{water}. We will replace P14.L26 with the following text: '*Without predetermination of the variables, we found the same ingredients as used in the Dalton model as the most important drivers of E_{water} at hourly and daily timescales*.'

- P14.L28: "To determine if the coefficients …" Without the results of the analysis it is impossible to assess the results. Either give the results or erase this sentence.

The result from the ANOVA analysis showed that with a p-value of 0.02, that the inclusion of the station (i.e. Stavoren or Trintelhaven) matters. So we cannot use the same model coefficients for both locations, and in other words: we cannot rule out that the sites are different (P14.L29). We will add the ANOVA table as Appendix, and adjust P14.L29: *'..., an ANOVA statistical analysis was performed (see Appendix ...). This analysis shows that inclusion of the station matters ($p < 0.05$). Therefore, we cannot rule out that the sites are different*.'

```
> anova(fit0, fit1)
Analysis of Variance Table

Model 1: LE ~ u_De + I(u_De^2)
Model 2: LE ~ u_De * station + I(u_De^2) * station
  Res.Df    RSS Df Sum of Sq      F  Pr(>F)
1    559 664256
2    556 652804  3     11452 3.2513 0.02151 *
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
```

- P16.L5-9: "The results for the location of …" This is a good analysis of your results. However be sure to be consistent. In your abstract you say that the model performs well.
Agreed. We checked for consistency and adjusted the abstract: '*Validation of the data-driven models demonstrates that a simple model using only two variables yields satisfactory results at Stavoren, with R^2 values of 0.84 and 0.67 for hourly and daily data, respectively. However, the validation results for location Trintelhaven fall short (R^2 values of 0.65 and 0.44 for hourly and daily data, respectively). Using only routinely measured meteorological variables leads to adequate performing simple models at hourly (R^2 = 0.79 at Stavoren, and R^2 = 0.51 at Trintelhaven) and daily (R^2 = 0.86 at Stavoren, and R^2 = 0.83 at Trintelhaven) timescales.*'

Discussions

You need to improve your discussions and criticise your result in a more precise way. You could be more exhaustive and include limitations (e.g decomposition of the radiation term, looking at the influence of other hydroclimate variables on the variance).

- P17.L7: You should be more precise and discuss the fact that you do not analyse each term of the radiation budget.
Agreed. We will remove the brackets around <<global>> at P17.L7, and we will add the following to P17.L9: '... figure 3). '*Note that the relation between E_{water} and other components of the radiation budget could not be studied, because of the lack of observations of these components. In combination with absent data on G, this prevented us from fully capturing the role of net radiation in the energy balance of the lake, and thus with the warming and cooling of the lake, which relates to evaporation through the water surface temperature.'*

- P18.L6: you can add Le Moigne et al 2016 as a reference.

Thank you for this suggestion. As far as we can see the reference mostly focusses on the impact of lakes on climate using the Flake lake scheme. It does not directly support the statement we make here about the combination of wind speed and vapour pressure gradient to estimate E_{water}. However, we will include this interesting reference to the introduction at P2.L3.

Patrick Le Moigne, Jeanne Colin & Bertrand Decharme (2016) Impact of lake surface temperatures simulated by the FLake scheme in the CNRM-CM5 climate model, Tellus A: Dynamic Meteorology and Oceanography, 68:1, DOI: 10.3402/tellusa.v68.31274

- P19.L10-13: It is impossible to review this part of the discussion as you don't provide the results. You should either erase the sentence or give the correlation plots.

Agreed. After consideration, we decided to erase these sentences as it does not contribute substantially to our storyline to add more graphs.

- As mentioned in the precedent comment (for P14.L22), you are working at short timescale and thus, the lake evaporation is dependent on the weather and the hydrological variability. Your discussion would be more complete by discussing these points.

We agree that this point should be noticed in the discussion section. In the precedent comment for P14.L22 we have added the suggested change to the text.

Conclusions

- P21.L19: In my opinion, your main contribution is the development of the statistical model. I would suggest to rephrase your conclusion in order to account for this.

Indeed the development of the statistical model forms a significant part of this study. However, the statistical model is used as a tool to explore the drivers of open water evaporation. We suggest to make the following adjustment at P21.L19: '*In this study, we investigated the dynamics and drivers of*

open water evaporation of Lake IJssel in the Netherlands through the development of a data-driven model.'

- P21.L20: Ok but this a general fact and this is not your main result.

We stated it there to provide a context for readers, but in order to kick-off with our main conclusions we will remove the following sentences: P21.L20 'We have shown....' till P21.L23 '....of open water evaporation'.

- P21.L26-28: Rephrase the sentences to be more precise on the result you use (if it's hourly or daily timescale). It is hard time following which are the R^2 you are presenting.

We agree with that and we will be more specific in our referencing to the timescales: P21.L26-28: '*Using the data collected in 2019 regression models for both location were developed. At the hourly timescale this resulted in R^2 = 0.74 and R^2 = 0.71 for Stavoren and Trintelhaven, respectively. Validation of these hourly models using the data collected during the summer of 2020 have shown that a simple data-driven model is able to explain large part of the hourly dynamics of open water evaporation (R^2 = 0.84 and R^2 = 0.65 for Stavoren and Trintelhaven, respectively).'*.

Editorial comments

- Some sentences lack of consistency and readability. This is often the missing punctuation that is in cause. For example, look at P2.L12, P7.L17, P7.L24, P9.L24, P12.L3.
We will correct this.

- Be attentive to have consistency in the form you write the units. The general form is to separate units with a point, ex : m.s-1 .
HESS prescribes to have the units written with a space and exponent, e.g. W m^{-2}. We will check for consistency throughout the manuscript.

- « Focussing » should be written like this « focusing ».
Noted.

- P2.L8 : parametrise/parametrize and not « parameterise ».
Parameterize is widely used and also accepted by HESS. We will change to parameterize.

- P2.L26 : add a « to » → « and to represent ».
We will add this.

- P2.L33 and P7.L20: check the tense.
P2.L33: will change into: 'entered'
P7.L20: will change this into: 'we used'

- P4.L25 : use the English structure : 1,100 km^2
Agreed.

-P5. Figure1 : Increase police size of your scale as it is not readable. Moreover, you should add the label on the contour lines. On the right figure you can erase the y-axis as it is the same than the center figure. Green and white colors for the center and right panels are not adapted to understand where are the land and the water. Add labels to the windrose.
Thank you for this feedback. We will adjust this figure according to your feedback.

- P7.L8 : you should either use co-variance or covariance but not both throughout you paper.
Noted.

- P8.L9 : « variable(s) » instead of « variable(x) ».
Thank you for spotting this. Will be adjusted.

- P10 Table 1 : Be sure all the parameters are aligned in the first column.
We placed the indent there as T_{air}, $T_{air,climatology}$, and T_{water} are all belonging to the variable T.

- P12.L11: “are lacking”
Noted.

- Please consider improving your Venn diagram in order to gain in readability (police size).
The arrangement of all the intersections of the Venn diagram leaves less space for increasing the font size. We will do our best to gain readability.