Response to referee comment Anonymous Referee #3

We appreciate and would like to thank Anonymous Referee #3 for reading our manuscript and taking the time and effort to provide us with generally positive and constructive feedback. We will use this feedback to improve our manuscript. Please, find below our point-to-point response (comment of the referee in **black**, our response in **blue**).

Review of HESS manuscript #hess-2021-549
Title: Evaporation from a large lowland reservoir – observed dynamics during a warm summer

The manuscript analyses evaporation measurements by EC towers and provides simple regression models based on the routine observations to describe evaporation dynamics in Lake IJsell (Netherlands). It is well written and organized. However, there are some aspects in the study that need clarification or should be addressed by the authors.

• There are many studies showing that evaporation dynamics vary between different parts of inland water bodies arising from, for example, spatial variability of water depth or meteorological inputs (boundary conditions). Considering this, it is fine to have two different measurement stations at the middle of the water body (Trintelhaven) and at the border (Stavoren). However, a discussion on the effect of EC towers location on the observed evaporation dynamics from the water body is missing (not its comparison with terrestrial evaporation as mentioned in section 2.2).

This is a good point that is raised. We will add this to the discussion section of the manuscript.

• Vapor pressure at the air-water interface was estimated based on the surface temperature obtained from satellite imagery that often show biases. Was this checked?

Specifications of the Meteosat sub-skin water temperature product describe a bias of 0.5 Kelvin, where validation takes place with temperature measurements from drifting buoys. We argue that this small bias will not change our findings of the most important drivers (i.e. wind speed and vapour pressure gradient) of E_{water}. If at all, the bias in the satellite product might lead to slightly different observed dynamics in water temperature and thus vapour pressure, which potentially could lead to changes in the regression coefficients. However, we think this effect will be insignificant.

• From our own measurements of vertical water temperature in a shallow basin with 2 m depth, I can say water temperature at the surface, where evaporation takes place, is completely different with even 10 cm below. Thus I am not sure how water temperature at depth of 1.2 or 1.5 m could help for evaporation analysis, unless you have a temperature model to reproduce surface temperature.

We agree that indeed preferably the skin temperature of the water surface is used in the estimation of evaporation rates. That is why we have used the satellite product in the first part of our regression analysis. Additional to that, we were interested if other routinely observations in the Netherlands which are easily accessible would provide another reasonable source from which evaporation rates could be estimated. The good correlation (R^2 = 0.71 and R^2 = 0.94; based on summer 2019 at Stavoren and Trintelhaven, respectively) between sub-skin water temperature from the satellite product and water temperature measured at depth of 1.2 or 1.5 m gave rise to that.

• It is not surprising that neglecting thermal inertia of the water body (indicating the effect of radiation adsorption in depth) could make such considerable bias in the performance of models such as Penman's (see Friedrich et al. 2018: DOI:10.1175/BAMS-D-15-00224.1). Please see Section 2.4 of Zhao et al. 2020 (https://doi.org/10.1016/j.rse.2020.112104) accounting for the impact of G on evaporation estimates by Penman-type approaches.

As a response to the role of G on evaporation, we would like to refer to our response to specific comment 3 of referee #1.

• Why air temperature is not included in the analysis of section 3.4?

We did not include air temperature as a separate variable here because we chose to include its effect in the vertical vapour pressure gradient, where air temperature is integrated in.

• I believe radiation is a key component in shaping surface temperature that, in turn, defines vapor pressure gradient at the core of your simple regression model. In application of Eq. 10, how surface temperature is obtained? From measurements at depth of 1.5 m, satellite imagery, or solving for energy equation? Dalton-type models may look simple in representation but have difficulties associated with obtaining reliable surface temperatures, and of course wind function (especially in the context of the projected climate change).

We agree that radiation is shaping surface temperature, and thus affects the vapour pressure gradient. In turn, evaporation decreases surface temperature and affects vapour pressure gradient as well. They are interdependent. To answer to your question about equation 10: We did not include the Dalton model in our analysis in the form presented as equation 10. We have placed it her in the discussion section to make a comparison between the functional form of the Dalton model and the regression model that we have found. To be complete, in figure 3 where we did present the monthly average diurnal cycle of, amongst others, the Dalton model. There, we have used the temperature data originating from the satellite product to calculate the vapour pressure gradient. We will add about the difficulties associated with the Dalton model to the discussion section.