Interactive discussion on "Seasonality of density currents induced by differential cooling"

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Response to Referee #1

R (Referee): *This paper describes an experimental work aimed at studying the occurrence of thermal siphons in Rotsee, a shallow lake sheltered by the wind. "Thermal syphon" indicates a physical process driven by differential cooling mainly due to bathymetry, which has important ecological implications, enhancing the hydraulic exchange between the littoral and the pelagic zone. The Authors made use of 1-year velocity and temperature data in the shallower area to detach thermal syphons. They focused on the frequency of occurrence over the year and analysed the forcing data suitable to explain this seasonality.*

They developed a state-of-art experimental work, winding the situ descriptions of the phenomena, which are not so frequent in the literature, especially when aimed at investigating the process over the seasons.

A (Authors): We thank Referee #1 for the critical assessment of the manuscript. His/her comments about the methods and the presentation of the results will improve the clarity of the paper. We addressed them below.

RC1 (Referee's Comment #1): *The main limits of this contribution are the weak readability of the paper and the case-specific algorithm proposed to analyse the phenomena. With regard to the first aspect, my suggestion is to be much more concise in the text and in the figures, limiting the number of information to the most relevant ones, or alternative to help the reader to distinguish between the more and less relevant.*

AR1 (Authors Response #1):

- Regarding the readability of the paper, we will follow the reviewer's suggestion to make the text more concise by removing unnecessary information and moving details to the Appendix. We will also send the manuscript for English language editing, which should help to gain clarity. The figures will be improved, as explained in more detail in AR8 and AR9.
- Regarding the algorithm used to detect thermal siphon (TS) events, we acknowledge that it is lake-specific, as several criteria are based on threshold values that can vary between systems. Specificity of the algorithm has already been discussed in Sect. 4.5 (lines 560-563) and in Appendix C. However, such an algorithm can serve as a basis for detecting TS in other lakes. Hence, the study is not "case-specific" and the general structure of the algorithm (Sect. 2.4) can

readily be used in other systems. The only changes consist in adapting the threshold values and possibly modifying the filters to distinguish TS from other cross-shore flows. We will update Appendix C to better reflect this question. The lines 616-617 will now read: *"The general structure of the algorithm can be used in other systems. Yet, several criteria are lake-specific and must be adapted to the system of interest. We discuss the limitations of the algorithm below and provide suggestions of improvement."*

We also suggest adding the following sentence on line 562: *"The general structure of the algorithm (Sect. 2.4) can serve as a basis for detecting TS in lakes, by adapting the lake-specific criteria to other systems."*

RC2: *More specific suggestions are listed in the followings.*

Methods 2.1. The computation of the wind speed doesn't seem satisfactory for two reasons: 1- the location of the meteorological station is hardly well representative of the wind conditions over the lake's surface 2 – the methodology to derive wind data from the Lucerne station is not properly justified. Given the sheltering of this lake, a correlation between these sites' data is unlike and I am quite doubtful about the suitability of a neural network algorithm to estimate it, given the local character on the wind field. Despite I don't know the typical wind speeds at these site, I believe that in relative terms an error E_{RMS} of 0.67 m/s is high. On the contrary, the approach is valuable for the other variables. I think that these sources of uncertainty must be accounted and discussed. Actually I do not have any suggestion regarding the solution of problem 1, apart for discussing this limitation in case of absence of other suitable data. With regard to problem 2, instead, I believe that is necessary to introduce an uncertainty in the fluxes evaluation.

AR2:

- Regarding problem 1, we acknowledge that the wind speed can be different between the location of the meteorological station and the lake center (MB), although the distance between the two points is less than one kilometer. However, we think that our measurements are representative of the wind conditions over the nearshore plateau region, where thermal siphons are created (i.e., north-eastern end of the lake). The wind speed cannot only vary between the meteorological station and the lake center, but also all over the lake's surface. The spatial variability of the wind speed is a common problem for the in-situ estimation of heat fluxes in lakes, as it cannot be resolved by a single meteorological station. In the case of Rotsee, we do not expect a significant effect of the spatial variability of wind speed on the daily averaged heat and buoyancy fluxes, because of the small size of the lake and the prevalence of low wind conditions. The daily-averaged wind speed is indeed less than 1 m s^{-1} for 80 % of the days and less than 2 m s^{-1} for 95 % of the days. We propose to add a sentence about the assumption of spatial homogeneity on lines 154-155: "*We assume that the meteorological conditions and the heat fluxes are spatially uniform over the lake's surface (0.5 km^2).*"

- Regarding problem 2, we agree that estimating the wind velocity from the Luzern station introduces uncertainties, which we quantified by the root mean square error on lines 150-152. However, we believe that a Neural Network (NN) approach is the most robust method to correct the Lucerne data to Rotsee. The performance of NN for estimating the spatial variability of wind speed has been demonstrated by Philippopoulos and Deligiorgi (2012). For other examples of studies using this approach, we refer to our answer AR6 to Referee #2.

  To illustrate the performance of NN in Rotsee, we compare the estimated wind speed from NN with the measured wind speed in Lucerne and Rotsee stations over a month (Fig. R1.1). The period shown in Fig. R1.1 is not part of the NN training period. Although wind speed is larger in Lucerne than in Rotsee, a coherent correlation between the two sites is observed for most of the wind events. The NN approach reproduces well the trends and the averaged magnitude of wind speed in Rotsee. It allows a better estimation of wind speed than the Lucerne measurements by decreasing the root mean square error from $E_{RMS} = 1.6$ m s^{-1} to $E_{RMS} = 0.67$ m s^{-1}. The distribution of wind speed in Rotsee is also better reproduced with the NN estimates than with the Lucerne data (Fig. R1.2).

  The effects of wind speed on the sensible and latent heat fluxes are taken into account in the calibration function f (McJannet et al., 2012; Fink et al., 2014). Several empirical expressions for f are available in the literature. We used $f = (2.33 + 1.65U_w)L_{fetch}{}^{-0.1} + 0.26(T_w - T_a)$ based on McJannet et al. (2012) and Fink et al. (2014), with U_w the wind speed at 2 m height, $L_{fetch} = 2500$ m the lake fetch, T_w the lake surface temperature and T_a the air temperature. This expression for f was selected by comparing the estimated heat fluxes with the observed change of heat content at MB. An error of $\delta_w = 0.67$ m s^{-1} in the wind speed leads to an error of $\delta_f = 1.4$ W m^{-2} mbar^{-1} in the function f, which is lower than the uncertainty of f (differences between estimates of f can reach 3 W m^{-2} mbar^{-1} depending on the empirical formula used). The resulting errors in the surface heat flux H_{Q_0} and surface buoyancy flux B_0 depend on the meteorological forcing. From the yearly averaged meteorological data, the errors are $\delta_{H_{Q0}} =$ 6.1 W m^{-2} and $\delta_{B_0} = 2.1 \times 10^{-9}$ W kg^{-1}, which is 5 % of the yearly averaged H_{Q_0} and B_0. We will include the error on the heat fluxes on line 152: "*The uncertainty in the estimates of wind speed and relative humidity leads to an average uncertainty of 5 % and 3 % of the surface heat fluxes (Sect. 2.2), respectively.*"

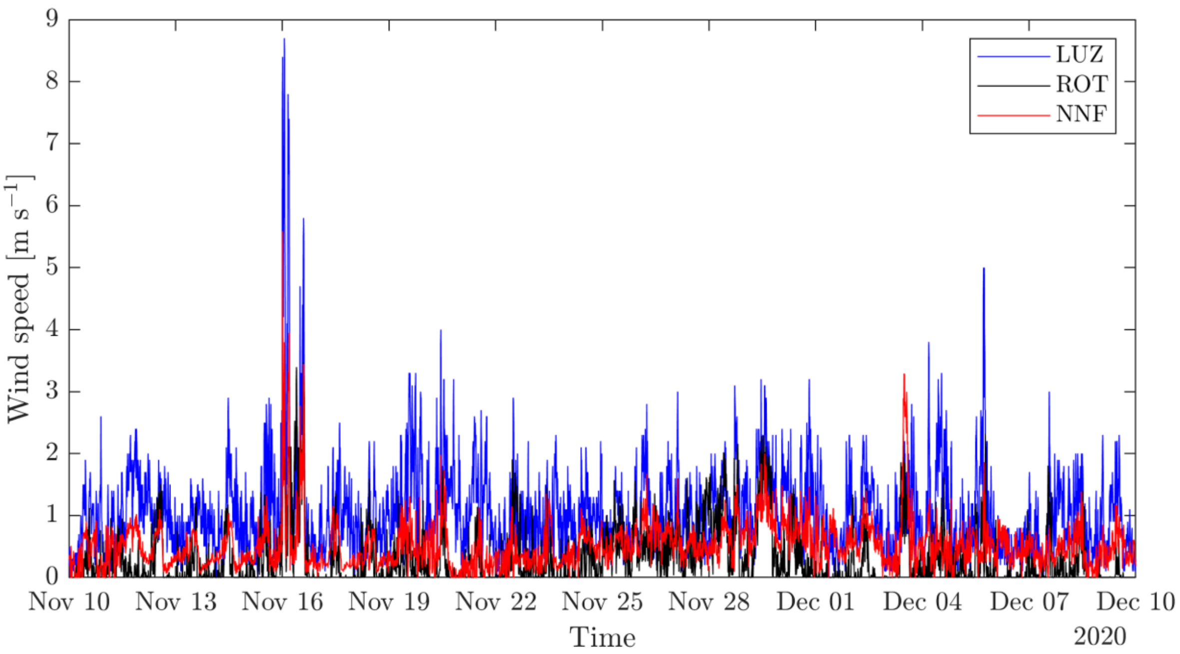

Figure R1.1: One month-long time series of wind speed measured by the Lucerne (LUZ) and Rotsee (ROT) stations, and estimated from Neural Network Fitting (NNF) from November to December 2020.

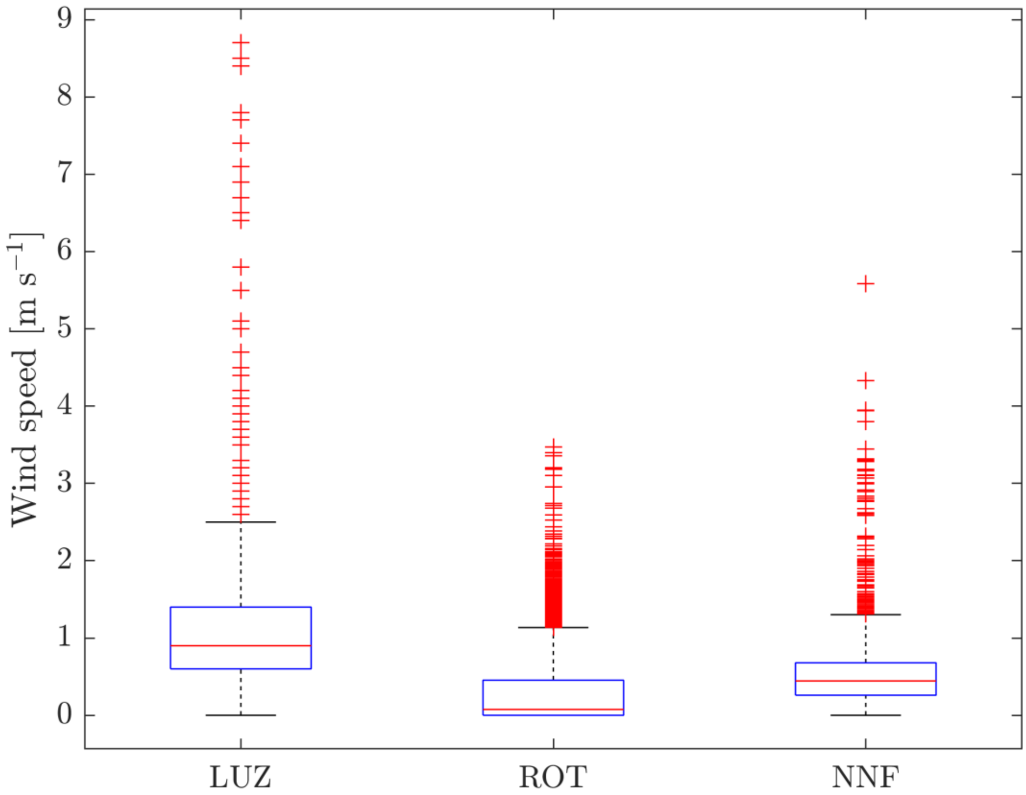

Figure R1.2: Box plots of the wind speed measured at the Lucerne (LUZ) and Rotsee (ROT) stations, and estimated from Neural Network Fitting (NNF). The box plots are based on the dataset of Fig. R1.1.

References:

Fink, G., Schmid, M., Wahl, B., Wolf, T., and Wüest, A.: Heat flux modifications related to climate-induced warming of large European lakes, Water Resour. Res., 50, 2072–2085, https://doi.org/10.1002/2013WR014448, 2014.

McJannet, D. L., Webster, I. T., and Cook, F. J.: An area-dependent wind function for estimating open water evaporation using land-based meteorological data, Environ. Modell. Software, 31, 76–83, https://doi.org/10.1016/j.envsoft.2011.11.017, 2012.

Philippopoulos, K. and Deligiorgi, D.: Application of artificial neural networks for the spatial estimation of wind speed in a coastal region with complex topography, Renewable Energy, 38, 75–82, https://doi.org/10.1016/j.renene.2011.07.007, 2012.

RC3: *Methods 2.2.*

L 160-161. Is the SW measured or parametrized? From section 2.1 it seems that it is measures but then in 2.2 it seems to be parametrized. Being a widely available parameter, I do not see the need to compute it.

AR3: Incoming solar radiation reaching the lake's surface (R) is directly measured (Sect. 2.1, line 140). However, a part of R is reflected at the lake's surface and is not included in the shortwave radiation entering the lake $H_{SW,0}$. To compute $H_{SW,0}$, the albedo of direct and diffuse solar radiation is parametrized as a function of the cloudiness (Fink et al., 2014).

Reference:

Fink, G., Schmid, M., Wahl, B., Wolf, T., and Wüest, A.: Heat flux modifications related to climate-induced warming of large European lakes, Water Resour. Res., 50, 2072–2085, https://doi.org/10.1002/2013WR014448, 2014.

RC4: *L172. Are there measurements to support the S value? Did you perform any sensitivity to assure that possible variations had no effect on your evaluations at a seasonal scale?*

AR4: Yes, we have salinity estimates from conductivity profiles collected over the year. The surface salinity increases from summer to winter by approximately $\Delta S \approx 0.5$ g kg^{-1} due to vertical mixing between the epilimnion and hypolimnion. The associated change of density is around $\Delta_S \rho \approx 0.4$ kg m^{-3}, which is almost one order of magnitude lower than the seasonal change of density due to surface temperature $\Delta_S \rho \approx 3$ kg m^{-3}. We suggest adding the following sentence on line 172: "*We* *assume a constant salinity over the year, as the seasonal change of surface water density due to salinity is one order of magnitude lower than the seasonal change due to temperature*."

RC5: *L176. From this paragraph it seems that HQ0 is always a negative loose term, while the shortwave is the only one term that contributes to heating. On the contrary LWin and HC can be positive too. In general the way to manage the signs of the fluxes in this section terms is a bit confusing. I suggest to reason in term of H0net and BOnet only (1+2 eq), without distinguishing between SW and other terms. What is important to verify is whether the net flux is positive or negative. This suggestion should be extended to the other sections of the paper.*

AR5: All the heat fluxes are defined positive in the upward direction (cooling), as explained on lines 154-155. Even if some of the surface heat fluxes can be negative (heating) as mentioned by the reviewer, the total surface heat flux H_{Q_0} remains indeed positive for most days. This continuous surface cooling is mainly due to the loss of longwave radiation. We believe that the confusion comes from lines 175-176 where we oppose surface cooling to radiative heating. We propose to modify this sentence as follows: "*The net buoyancy flux at the surface is $B_{0,net} = B_0 + B_{SW,0}$. $B_{0,net} > 0$ indicates a destabilizing buoyancy flux (net cooling) whereas $B_{0,net} < 0$ indicates a stabilizing buoyancy flux (net heating).*"

We agree with the reviewer regarding the use of the net heat and buoyancy fluxes. The cooling and heating phases must be determined from $H_{0,net}$, which is directly related to $B_{0,net}$ (lines 176-178). However, the driving force of TS is surface cooling, expressed by a destabilising surface buoyancy flux B_0. Distinguishing B_0 from the radiative (penetrative) buoyancy flux is required to determine the convective velocity scale (Fig. 4) and the transport scaling formulae (Sect. 2.6). The use of $B_{0,net}$ and B_0 is already explained on lines 176-178.

RC6: *Methods 2.4. In the methods aimed at detaching the thermal syphons I would have expected to see the vertical component of the velocity as a target variable, in particular before the beginning of the event. Is there any reasons why you did not mention it? Given the uncertainty on the wind data, I think it could be a better way to distinguish between thermal syphons and wind driven flow (see e.g. Fer et al. 2002).*

AR6: Thank you for the suggestion. The vertical velocity measured by the ADCP has indeed a different signature between TS events and wind-driven flows and we initially tried to use it in the "wind filter" of our algorithm. TS events are characterized by convective plumes with an alternating upward and downward vertical velocities (Fig. 3c), whereas wind-driven cross-shore flows are associated with strong downwelling at MT, when they are directed in the x-direction ($U_x > 0$). The challenging aspect of this approach is the definition of the criterion used to distinguish the two different signatures in the vertical velocity. An option is to use a threshold value for U_z (which is larger for downwelling than convection) or to focus on the change of sign of U_z over a certain period. Regarding reproducibility, these criteria involve arbitrary threshold values that are not necessarily physically grounded and could be more system-dependent than our criterion based on the Monin-Obukhov length L_{MO}. We also think that vertical velocities can be more difficult to measure in the field than L_{MO}, as they require high resolution ADCP data. Additionally, the wind filter based on L_{MO} can be applied a priori to predict the occurrence of TS on a specific day. Although there is uncertainty in the wind speed estimation, our wind filter correctly discarded all the cross-shore flows associated with downwelling.

We are not sure what the reviewer refers to in Fer et al. (2002). To our understanding, Fer et al. (2002) did not use the vertical velocity to distinguish between TS and wind-driven flows. They used the ratio z/L_{MO} to study the effects of wind on TS (with z the depth of interest, see their figure 7), which is similar to our approach. Yet, we still think that using vertical velocities could be an interesting approach to try in the future and we will mention it on line 641 in Appendix C: "*Additional filters could be implemented to distinguish between TS and wind-driven cross-shore flows, based for example on high-resolution vertical velocity measurements, observed oscillations of the thermocline (e.g., wavelet analysis), estimates of the period of internal waves and identification of upwelling events (e.g., Wedderburn and Lake numbers) (Imberger and Patterson, 1989).*"

Reference:

Fer, I., Lemmin, U. and Thorpe, S. A.: Winter cascading of cold water in Lake Geneva, J. Geophys. Res., 107(C6), 3060, https://doi.org/10.1029/2001JC000828, 2002.

RC7: *Table1. Separate the extremes of the range with a "-" in place of a ",". Why no range for Ux and Uz?*

AR7: We used "," to avoid confusion with the minus sign. But we will follow the reviewer's suggestion and use "-" for the ranges in Table 1, with brackets around negative values. We will also add the range of values for U_x and U_z: $[(-0.05) - 0.07]$ m s^{-1} and $[(-0.01) - 0.01]$ m s^{-1}, respectively.

RC8: *Figure 3,4,7,8. The figures of this paper are too much dense of information. The effort to make them fully informative has the counter-effect to confuse the reader with too much data and is not efficient in highlighting a clear message. Make an effort to make the figures clearer, with less but more direct information, and eventually reduce the number of figures. (Fig. 8 in particular is really hard to follow)*

AR8: We appreciate the critical assessment of the figures provided by the reviewer. We acknowledge that our figures provide a lot of information but we tried to provide a detailed description in each caption. The two other reviewers are both very positive about the figures (R2: "*I (...) found all of the* *figures engaging*", R3: "*The writing and presentation are mostly very good.*"). We will still try to improve the clarity of Fig. 3, 4, 7 and 8 as follows:

- Figure 3: see the specific answer AR9 below.
- Figure 4: we suggest removing the grey error bars in Fig. 4a to improve the readability.
- Figure 7: we will better indicate the month for each row of subpanels.
- Figure 8: we propose to move Fig. 8b to Appendix B as it adds complexity and it is not directly related to Fig. 8a. We will replace it with a schematic illustrating the different time scales over the cooling phase (see Fig. R2.2 in the response to Referee #2).

RC9: *Fig. 3. The first panel is useless. In the second panel limit the plot of B0net. Velocity and temperature contours together limit the readability. Look at Fig. 3 of Fer et al. 2002 as an example of a good representation of a single event: a single line of Ux and Uz is much clearer. Finally the contours of temperature between 13.00 and 17:00 looks like affected by an error in the interpolation. Do you have enough thermistors? If so, how do you explain the different pattern? How do you explain the rapid changes in signs of Uz? Please comment.*

AR9:

- Fig. 3a: We do not think that this panel is useless, as it illustrates the seasonality of the diurnal cycle shown for a specific day in the three other panels. In particular, Fig. 3a shows the seasonality of (1) the duration of the heating and cooling phases and (2) the magnitude of the forcing. The seasonality of the diurnal cycle is a key aspect to understand how the occurrence (Sect. 4.2) and the flushing period (Sect. 4.4) vary over the year. We will add more references to this panel in the text. We also think that the reviewer's comment comes from the fact that Fig. 3a seems disconnected from the three other panels. We will better link them by clearly indicating in Fig. 3a where the diurnal cycle of Fig. 3b-d appears.
- Fig. 3b: Showing only $B_{0,net}$ does not provide enough information in our view. $B_{0,net}$ implicitly indicates the cooling and heating phases, associated with destabilising and stabilising surface buoyancy fluxes, respectively. However, B_0 and $B_{SW,0}$ are relevant to demonstrate that the main driver of the diurnal cycle is the solar radiation, and not the temporal change of surface cooling (B_0 remains positive all day). To better emphasize $B_{0,net}$, we will decrease the linewidth of B_0 and $B_{SW,0}$.
- Fig. 3c-d: The goal of this figure is not only to identify a TS event as in Fig. 3e of Fer et al. (2002b), but also to provide the main characteristics of the convective circulation (opposite cross-shore flows, thickness of TS, region of maximum velocity) and to indicate the region where the transport is calculated (blue curves in Fig. 3d). The latter requires to show velocity contours, and not only depth-averaged velocities. This is similar to the event presented in Fig.

2 of Fer et al. (2002a). Note that Fig. 3e of Fer et al. (2002b) focuses on the cross-shore velocity U_x and the along-shore velocity U_y. It does not include the vertical velocity U_z as in our Fig. 3c.

We also want to keep the isotherms in Fig. 3d, as they show the stratification induced by the density current. To improve the readability, we suggest increasing the spacing between the isotherms and showing them in gray. The dense surface temperature contours during the second day are due to the strong surface heating captured by the surface thermistor. The temperature has been linearly interpolated between the thermistors and there is no thermistor between the surface and 3 m depth due to rowing restrictions (lines 124-126 and Table A1), which leads to dense isotherms down to 3 m depth (2 m height above the bottom). We will add a tick on the y-axis to indicate the location of the surface thermistor and we will mention the linear interpolation of temperature in the caption.

Vertical velocities in Fig. 3c have a temporal resolution of 15 min, as explained in the figure caption. We did not interpolate them over time: one value is shown at each depth every 15 min. The "rapid" changes in signs of U_z come from the upward-downward motion of the convective plumes every 15 min. The duration of half of a convective overturn can be estimated as $\tau_{conv} = h_{MT}/U_z$ with $h_{MT} \approx 4$ m the depth at MT and $U_z \approx 0.005$ m s^{-1} the velocity of convective plumes. This gives $\tau_{conv} \approx 13$ min, which is less than the temporal resolution of Fig. 3c. The convective plumes can thus have opposite directions between two consecutive measurements.

References:

Fer, I., Lemmin, U. and Thorpe, S. A.: Contribution of entrainment and vertical plumes to the winter cascading of cold shelf waters in a deep lake, Limnol. Oceanogr., 47(2), 576–580, https://doi.org/10.4319/lo.2002.47.2.0576, 2002a.

Fer, I., Lemmin, U. and Thorpe, S. A.: Winter cascading of cold water in Lake Geneva, J. Geophys. Res., 107(C6), 3060, https://doi.org/10.1029/2001JC000828, 2002b.

RC10: *Results 3.4. The R2 values are really low, I would limit the analysis only to the variables which show at least a trend (not the case of tauf for example). In the conclusion you defined "robust" these relationships , but these R2 do not support these conclusions. I would be more cautious to base the conclusions on the basis of these results.*

AR10: We acknowledge that the R^2 values are low, which is not surprising knowing the natural variability of the process. We attributed the strong variability of q_{avg} and τ_F to the fluctuations of the thickness of TS, as discussed in Appendix C (lines 655-663). Despite the scatter between days, the four variables of Fig. 6, including τ_F, show a linear trend. This is confirmed by the low p-value ($p_{val,F} < 10^{-53}$ for the four variables), which indicates that the slope of the linear fits is significantly different from zero. We agree with the reviewer that the term “robust” may be excessive: we will remove it from the conclusion. Lines 588-589 will now read: “*This study provides a field validation of laboratory and theoretically based scaling.*”

RC11: *References. A careful review of the references is needed (for example Rao and Schwab, Meyers and Dale are not present)*

AR11: Thank you for the comment, we apologize for this issue. We realized that our reference management software did not work properly. We will add the following missing references:

- Fink, G., Schmid, M., Wahl, B., Wolf, T., and Wüest, A.: Heat flux modifications related to climate-induced warming of large European lakes, Water Resour. Res., 50, 2072–2085, https://doi.org/10.1002/2013WR014448, 2014.
- McJannet, D. L., Webster, I. T., and Cook, F. J.: An area-dependent wind function for estimating open water evaporation using land-based meteorological data, Environ. Modell. Software, 31, 76–83, https://doi.org/10.1016/j.envsoft.2011.11.017, 2012.
- Meyers, T. and Dale, R.: Predicting daily insolation with hourly cloud height and coverage, J. Climate Appl. Meteor., 22, 537–545, https://doi.org/10.1175/1520-0450(1983)022<0537:PDIWHC>2.0.CO;2, 1983.
- Rao, Y. R. and Schwab, D. J.: Transport and mixing between the coastal and offshore waters in the Great Lakes: a review, J. Great Lakes Res., 33, 202–218, https://doi.org/10.3394/0380-1330(2007)33[202:TAMBTC]2.0.CO;2, 2007.