

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1  
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## **Comment on hess-2021-195**

Anonymous Referee #1

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Referee comment on "Seasonality of density currents induced by differential cooling" by  
Tomy Doda et al., Hydrol. Earth Syst. Sci. Discuss.,  
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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 detect thermal siphons. 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.

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.

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 unlikely 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.

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.

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?

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.

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).

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

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)

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  $U_x$  and  $U_z$  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  $U_z$ ? Please comment.

Results 3.4. The  $R^2$  values are really low, I would limit the analysis only to the variables which show at least a trend (not the case of  $\tau_{uf}$  for example). In the conclusion you defined "robust" these relationships, but these  $R^2$  do not support these conclusions. I would be more cautious to base the conclusions on the basis of these results.

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