Reply to Reviewer 4

The paper contains very important information on evaporation from the Dead Sea that should be eventually published for the benefit of the scientific and water management communities. The paper presents measured heat fluxes using eddy covariance sys- tem over a year; the eddy covariance data is presented with a solid data analysis. In addition the paper evaluates the validity of common used indirect estimations of evap- oration from the Dead Sea, which is very important for extending the flux estimates when EC is not available. However I think that at the present form the paper cannot be published in HESS due to the following reasons:

The authors state that (end of P3) "To measure the energy budget components of the water surface, a fully equipped energy balance station was installed ... ", however this is not the case. Using eddy covariance - latent and sensible heat are measured directly, but heat storage is not measured nor the net radiation. The radiometer in use is a 4 components CNR4 (P5 L4-5), but as can be seen in Fig 1b the two lower half spaces are directed to the ground not to water surface; this is also acknowledged later on, but this should clearly be stated upfront. Water temperature is calculated not measured, this is found later on in the paper, should appear upfront when declaring for fully equipped energy balance station. In the results it is stated that the heat storage is calculated as the residuum of the energy balance (Rn-LE-H), where Rn by itself is not really measured, again, this is not a fully equipped system. I think the paper should be written with emphasis on the existing data, which are very important and worth publication, but not declaring for measuring energy balance, this gives a wrong impression for the reader. As stated above, water temperature was calculated, not measured, termed TMO. TMO is used for the examining the equations of evaporation versus measured evaporation (e.g. table 1 and large portions of the figures). TMO is also used to determine the saturation vapor pressure at water surface temperature, Ew. Ew is used to determine the vapor pressure deficit and for the evaporation estimates. So both water temperature and Ew are not measured, but they have a very important role in the analysis and the conclusions of the paper. Again, I this think that it would be better to orient the paper to the existing information that was measured and analyzed and not to rely so much on the computed meteorological parameters. Typically, when EC measured evaporation is compared with the evaporation equations it is done based on measured meteorological parameters, not on computed parameters. Overall, due to these weaknesses, there is a gap between the actual measurements and the interpretations and conclusions. The scientific methods and assumptions should be better declared earlier and should appear clearer. I think that the first half of the title "Dead Sea evaporation by eddy covariance measurements" is good and rep- resenting the novel aspects of this work, but the second half of the title represents the weaker part of the paper

The authors thank the reviewer for the insightful review. We are sure they will help to improve the paper.
The reviewer raised 5 important points: Firstly, a fundamental statement that the comparison to the indirect methods has significant weaknesses, and furthermore four points related to specific parts of the content: 1) There is no fully equipped energy balance station. 2) TMO, which is important in the analysis is not measured but calculated and then used for the calculation of Ew and the vapour pressure deficit. 3) The analysis relies too much on calculated meteorological variables. 4) Methods and assumptions should be better declared.
The responses to the reviewers comments are provided here:

We understand the concerns of the reviewer regarding the indirect methods. There are uncertainties in the calculations of the indirect methods, but as EC measurements are often not available for the long-term assessment of evaporation it was one of the main goals and thus an important part of the paper to provide a reliable and scientifically sound method for long-term flux estimates in the future. Additionally, the results can serve as a method to calculate the spatial variability of evaporation over the water surface, when using it with an appropriate

model, which delivers high resolution information of wind velocity and vapour pressure deficit.
With the first part of the paper alone we would of course publish important and novel data concerning the evaporation of the Dead Sea, but the second part provides an important basis for future studies and work on the topic of Dead Sea evaporation and evaporation of similar land-water configurations. We therefore think that the comparison to the indirect methods should remain part of the paper.

1) With respect to the comment about the energy balance station, we agree with the reviewer that the sentence on P3: "...a fully equipped energy balance station was installed" can confuse the reader, as not all energy balance components of the water surface were directly measured. We will change this sentence and add a paragraph with an explanation what components of the energy balance of the water surface were actually measured and how we calculated the missing variables, like the reflected shortwave and outgoing longwave radiation, and the water surface temperature.

2) + 3) both refer to the calculation of TMO and further meteorological variables.
One main goal of this paper was to provide a measurement concept and possible post-processing methods, which includes the common problem of assessing missing variables, with which evaporation measurements at the shoreline can be realised. The problem that not all necessary variables for an analysis are measured is a common problem in the assessment of evaporation (e.g. Lensky et al. (2005) used bulk formulas to estimate longwave radiation, Giadrossich et al. (2015) used a model to estimate stream discharge to the lake). Therefore, we don't see the calculation of e.g. TMO as a weakness of this paper but as part of the possible methods to gain evaporation data from a station on land.

One option to derive the surface water temperature (SST) from satellite data, as suggested from Reviewer 1, was also not possible because of the following reasons:

- Nehorai et al. (2009) used MeteoSat Second Generation data to estimate SST from the Dead Sea. For retrieving the SST the operational SST algorithms could not be applied as they are calibrated to sea level and do not take the additional 421 m atmospheric layer in the Dead Sea valley into account. They derived the SST by calibrating their algorithm against in-situ measurements. As we did not have the necessary in-situ measurements of the SST we could not follow their procedure to derive the SST from satellite data.

- Furthermore, Nehorai et al. (2009) raised concerns, that on days where the Mediterranean Sea Breeze enters the valley in the afternoon the enhanced evaporation causes enhanced water vapour and thus a stronger absorption of thermal IR radiation which leads to a screening of the Dead Sea surface and thus incorrect estimates of the SST. For their studies they excluded all data with these conditions. This would lead to data gaps in the time series of SST especially during offshore wind conditions, meaning that no SST data would be available for the timesteps where it is needed as an input parameter for the regression model to calculate evaporation from the water surface.

- Another point why satellite data was not used is the need of a continuous time series. Satellite data can not be used for cloudy conditions. So especially for the winter months cloud cover would reduce data availability significantly.

Because of the aforementioned problems using satellite data, we followed the advice of another paper from Nehorai et al. (2013), which shows that "SST is highly correlated to air temperature (0.93-0.98) in all seasons". Based on these results we used the similarity approach to calculate surface temperature from air temperature.

4) regarding the methods and assumptions we want to refer to answer 1). We will add a better description of the objectives, assumption and methods used to the introduction and section 3 "Data and Methods". We will e.g. explain what data was not measured and how it was calculated (e.g. the calculation of the net radiation.)

References

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