

## ***Interactive comment on “Dead Sea evaporation by eddy covariance measurements versus aerodynamic, energy budget, Priestley–Taylor, and Penman estimates” by Jutta Metzger et al.***

### **Anonymous Referee #4**

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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 system 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 evaporation 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:

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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 ( $R_n - LE - H$ ), where  $R_n$  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,  $E_w$ .  $E_w$  is used to determine the vapor pressure deficit and for the evaporation estimates. So both water temperature and  $E_w$  are not measured, but they have a very important role in the analysis and the conclusions of the paper. Again, I 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 representing the novel aspects of this work, but the second half of the title represents the

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weaker part of the paper.

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