Reply to Reviewer 3

The paper shows, for the first time, results of direct annual evaporation (E) measurements from the Dead Sea (DS) based on eddy covariance (EC) technic Understanding the annual and the short-term dynamic of the lake evaporation rate is important scientifically in many aspects, for the regional managers and for the future fate of the whole region. The paper is a clearly written, covering both measurements aspects and evaporation modeling aspects over free water body in exceptional conditions, and one can assume that the measurements were carried under very harsh conditions. Last, there are not many E measurements over water bodies that are based on eddy covariance technique and are comparing measurements results versus different evaporation rate models as the Authors presented here. Having said that, there are a few significant points the Authors need to address before any publications.

The authors thank the reviewer for the insightful review. We are sure they will help to improve the paper. Responses to individual comments are provided below. Reviewer's comments are in italic.

1 Major Comments

C1) Comparing annual evaporation results with previous estimation. Comparing to previous works need caution which the Authors have to mention and discuss, including; A. The change in the water level likely changed as well the DS surface area between the different estimation years (e.g., in the case of Stanhill 1994 the lake level was probably 30 m higher and surface area much larger). B. Changes of the climatic conditions due to large-scale changes as well as due to the lake shrinkage. The Authors already mentioned the rapid changes in the regional Persian trough frequency. C. Likely salinity changes over the years and possibly also the amounts of water removal to the mineral production pools in those years? And D. This work is based on a single measurement year that the Authors mentioned as a relatively wet one

Thank you for this comment. We agree that there are differences between the conditions of the former studies and our study. Climate and weather conditions, salinity and water level obviously influence the evaporation rate. We will point this out in the discussion and take your remarks into consideration when comparing our annual evaporation rate with former studies. Nevertheless, our main goal was not to rate former results of yearly evaporation amounts. Our main aim was to provide information on the short-term and intra-annual variability of evaporation as this was so far not provided by other studies and to provide a measurement concept and post-processing methods with which evaporation measurements at the shoreline can be realised.

C2) H and L_v calculations (section 3.1) were needed for the energy budget models (as in Tab1). And I assume, though not clearly presented, that ET was derived directly from EC evapotranspiration calculation, not from L_v? However, figure A1 is important in showing that compared with pure water, saline water L_v is lower for temperature higher than 22C , which likely means that for most times of the year L_v of Dead Sea water is lower than that of a pure water. In this respect, the sentence in L27, page P5 is confusing and future warming and increase water salinity will possibly increase E?

Yes, the evaporation was derived directly from the EC measurements.
If future warming and an increase in water salinity will increase evaporation can not be concluded from Fig. A1. These measurements were only conducted for the given salinity of the Dead Sea in 2014 and do not capture changes through salinty increase. There are two factors determining the L_v: 1) Temperature. It's correct that the data show that L_v of the Dead Sea is most of the time lower than L_v of pure water in case of water temperatures

larger 22°C. 2) Water salinity. It can be assumed that if water salinity increases, L_v increases as well (Salhotra et al., 1987). Yet, it is unkown how these two factors will balance each other in the future. For conclusions about the future development of the evaporation due to L_v changes studies with different salinities, also considering the chemical composition of the Dead Sea water, have to be conducted.

C3) Gap filling model for E values when wind direction is coming from the land enhances considerably the total evaporation, especially during the afternoons. However, this model uses VPD (and wind speed) derived from humidity values of air coming from the lake. While the humidity of the land air is probably lower compared to wind coming from the lake. But, it is likely that RH of this dry air increases as it is blowing over the lake for some distance., Thus VPD and E should decrease. Shouldn't such effects be estimated, considering its large effect on E? Do the Authors have any information on the RH difference between the two sides of the lake (e.g., west vs. east) for wind blowing to either directions?

The effect of a possible VDP decrease with increasing distance from the shoreline couldn't be directly estimated, but we made following observations: 1) We have seen in the data that the strong westerly winds are connected with high turbulence, and even rotor formation was observed. This means that vertical mixing and air mass exchange is enhanced and thus VDP decrease should be low. 2) The fetch of the station is around 600 m. In our opinion the decrease of delta_e within such a distance is not very strong considering the turbulent mixing. 3) Evaporation has a stronger dependence on wind velocity than on VDP which makes the influence of VDP variations on the results weaker.
The second question of the reviewer was if we have data from both sides of the lake. We don't have information on the VDP variation over the lake or from the eastern shore to validate our assumption. We agree with the reviewer that VDP variations are an uncertainty, but as stated in the above paragraph we don't think that it has an large effect.

C4) Combining or incorporating variables with previous works that have been carried out over the DS in the past to check estimations and assumptions. For example, I found published works on DS surface temperature (Tom) measurements, and others on the lake heat storage on different time scales. I am wondering why the Authors did not refer to this data? Δe is highly dependent on Tom and close to the shore Tom is warmer than in the open sea, thus it would be valuable if the authors could compare their estimations with independent measurements and its effects on E estimation.

Thank you for your comment. We used the published works on DS surface temperature as a basis for our method. E.g. Nehorai et al. (2013), showed that "SST is highly correlated to air temperature (0.93-0.98) in all seasons". Based on these previous results we used the Monin-Obukhov approach to calculate DS surface temperature from air temperature. Concerning the heat storage we could not find suitable data to directly use as input for our calculations and a direct comparison to independent measurements over the open sea is unfortunately not possible, as we don't have such measurements or access to such data sets.

5.This leads to the last main point: The basis for the uncertainty around E (82.2 mm) is unclear. For ecosystems over land, it is generally assume to be 10%; is it about the same here or? However, although the uncertainty value is about 8% of E it is likely still a substantial large number for water management of the region. Can Authors suggest ways to reduce this in future activities?

The uncertainty of the total evaporation amount (88.2 mm) contains the uncertainty due to the gap filling method. Gaps were filled using the median evaporation of the corresponding time step of the respective month and the uncertainty of this method was estimated using the Median absolute deviation (MAD) for the used time step (described in Sec. 4.3). The uncertainty due to the gap filling procedure accounts for 81.2 mm of the 88.2 mm uncertainty. The rest of the uncertainty (7 mm) stems from the regression model. Here, the prediction error given by the MCCV was used. To account for the highest possible uncertainty of the regression model the prediction error of the MCCV with randomly chosen validation sectors $er_s = 4.79\%$ was used.
The uncertainty could further be reduced by finding another method to fill the gaps. The use of a median evaporation cycle naturally results in relatively large MADs, as evaporation varies from day to day. Nevertheless, we choose this method instead of the often used interpolation, as interpolating would in some cases not depict the

real diurnal cycle. E.g. if we would use linear interpolation between the 18 LT and 24 LT value in Fig. 4 we would completely miss the diurnal maximum. Another way to reduce the uncertainty would be reducing the gaps in the data set itself. Gaps in the data set were caused by: malfunction of the system (2.4% missing data), precipitation events and problems with the radiation source (signal strength was too low, 10%), and quality control (integral turbulence characteristics and steady state test after Foken (1999), 9.2%). For example, shorter maintenance intervals could reduce the amount of data missing through system malfunction or problems with the radiation source, which were mainly caused by the very harsh conditions (see also answer to minor comment 2). However, precipitation events or conditions were the criteria of the EC method (fully developed turbulence and steady state) are not fulfilled can not be controlled.

2 Detailed Comments

1.L. 9 p. 3. I would look for additional citation(s) for the EC approach reliability to measure E over water bodies.
We will add further citations.

2.Is the IRGASON a close or open path IRGA? And generally, did the researcher had any problems with the presumable high rusty environment down there, with salt particles etc.? It is an open path instrument. Through the harsh environment the windows of the IRGASON got dirty in a short time, which influences the signal strength of the radiation source. Through these conditions short maintenance intervals of about 3-4 weeks were necessary.

3.Heat storage in section 4; can the Authors add 'zero' line in Figure 2, ΔQ value. The impression from inspecting that figure is that the annual value deviate considerably from zero? Is it due to negative heat transfer (e.g., by rain)?
We will add the zero line to Figure 2. Yes, there is a positive net heat storage when summing over the whole year. Several other studies like (Stanhill, 1990; Anati et al., 1987) already documented this and the thereby caused steady increase of the lake temperature over the last couple of years is also documented (e.g Hecht and Gertman (2003)).

4.Please add the units for MD and std in Table 6.
will be added accordingly

References

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