Reply to Reviewer 1

1 General Comments

I think this paper deserves publication because the dataset is particularly interesting. As said by the authors, it is the first time that one year of turbulent atmospheric fluxes measured by the eddy-correlation (EC) method has been presented for the specific area of the Dead Sea. The data have been professionally processed. They can efficiently be used to assess several parameterizations generally used for long-term measurements when the EC method is not available. The originality is that the authors provide several levels in the parameterizations, according to the measurements that can be performed. However the paper requires some important corrections before being published. I try to describe them below. Some corrections, related to the methodology, are essential. Other are secondary and consists in numerous details that could be improved.

Thank you for the very detailed and 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.

2 Specific Comments

Methodology : I think the difficulty comes from the fact you make a local measurement over ground (with the EC method) while you would like to include the close environment in the driving parameters of the turbulent fluxes, considering that the air that is advected on the measurement site is (most of the time) characterized by the water surface temperature and water surface vapor partial pressure. I am not against the idea, but I think the way you deal with this assumption is not always correct. I am aware that you want to prove that measuring on the headland, very close to the seashore is equivalent to measuring with a raft in the middle of the sea, but I am not totally convinced. Another issue is the fact that you address different time scales for the energy budget you consider, without saying accurately which timescale you refer to.

C1) When measuring the latent heat flux LE at level 6m with the EC, the Lv value (kJ/kg) that has to be used to change the evaporation rate $\overline{w'a'}$ into a flux is that of the air, i.e. 3148.4-2.37 Ta and not Tw as you use in Eq. 3. Ta is the air temperature in K. Even if the evaporation takes place at the water surface as you mention p 6, line 3, LE is assumed to be constant in the surface layer (in fact, not to vary more than 10% of its surface value).

Thank you for this comment. As Lv is a water specific variable and defined by the temperature of the liquid, in our opinion Tw is the appropiate variable to calculate Lv. The literature equation $Lv = 3148.4 - 2.37 \cdot T$ is derived via the Clausius-Clapeyron equation, by measuring water temperature and saturation vapour pressure. This results in the enthalpy of vaporization. Dividing the enthalpy of vaporization by the molar mass of water gives us the latent heat of vaporization. So it does not include air temperature measurements and is only a function of water temperature.
When using Lv to convert the evaporation rate $\overline{w'a'}$ into LE, which is the energy used to evaporate the water at the surface, from a physical point of view, the water surface temperature and not air temperature has to be used as Lv is a water specific variable Lv=fct(Tw).

This unique Lv value should be used for both offshore and onshore winds. In fact an internal boundary layer develops either inland or offshore, depending on the wind direction. Perhaps you should mention it and clarify the parameters you use in both situations. [The only opportunity to use Lw as you define it, would be to take *into account the heat loss of the water due to water evaporation, as shown by Giadrossich et al., 2015 in their eq (2). This eq. applies to the energy budget of the whole sea, and not the local energy budget that you quantify at the EBS].*

As in this paper we are only interested in fluxes from the water surface only fluxes measured during onshore wind conditions are used, because then the source area of the flux is over water. All flux measurements for offshore wind conditions are neglected as they represent the fluxes from the land surface. To fill the so created gaps in the time series the regression model is used.
At this point we take advantage of the very periodic wind systems at the Dead Sea. There are only very short time frames with westerly offshore winds where the flux measurements have to be neglected and therefore a large data set with onshore wind conditions is available to establish the regression of the latent heat flux from the water surface with wind velocity and water vapour pressure deficit. With this so gained regression we calculate evaporation from the water surface for the time steps with westerly winds as no measurements of the fluxes from the water surface for this conditions are available. With this procedure we get the full diurnal cycle of the evaporation from the water surface, just as it would be measured with a raft station in the lake. As only flux data for onshore wind conditions are used, $\overline{w'a'}$ is always converted into LE with Eq. 4:

$$Lv = 5150.6561 - 13.9530 \cdot T_w + 0.0162 \cdot T_w^2$$

(This equation is slightly different to Eq. 3, which is for pure water, as the salinity of the water also influences the latent heat of vaporization.)

For the investigation of the energy balance of the land surface no data from this station is used as we have another station further inland, which is not affected by the water surface. The results of the energy balance of the land surface are not discussed here as they are beyond the scope of this paper.

We will revise the paper to make the work flow and the used equations and parameters clear. Thank you for pointing this out.

C2) When discussing the various models you apply to your data, you use Tom-Ta or Ew-Ea. The latter should be replaced by E_{surf} - Ea, with E_{surf} (or another name) standing for the water vapor partial pressure at the surface (water or ground). The former, by T_{surf} - Ta:

- *For onshore winds, as the source area is over water, we think that the similarity profile you use in Appendix B, p26, to deduce the water surface temperature is not appropriate since the regressions you wish to establish in the following will depend on this profile. We suggest that you'd try to find independent remote-sensed measurements of the Dead Sea surface temperature instead, which is not exactly the air temperature at the surface, but is the closer you can find. If you do so, we am almost sure that the discrepancy between panels 2 and 3 in Fig. 2 will be larger. In that way, the models you will apply in the following will include independent measurements (since the temperature difference will not result from the similarity profile).*

  We agree that remotely sensed water surface temperature would be favourable as it is independent from the air temperature measurements. This was also considered and discussed when analysing the data but it was discarded because of the following reasons:

  - Nehorai et al. (2009) used Meteosat Second Generation (MSG) data to estimate water surface temperature from the Dead Sea. For retrieving the water surface temperature from the satellite data the operational SST algorithms could not be applied as they are calibrated to mean sea level and do not take the additional 421 m atmospheric layer in the Dead Sea valley into account. They derived the water surface temperature by calibrating their algorithm against in-situ measurements. Unfortunately, we did not have the necessary in-situ measurements of the water surface temperature to follow their procedure to derive the water surface temperature from satellite data.
  - Furthermore, Nehorai et al. (2009) raised concerns, that on days where the Mediterranean Sea Breeze, a strong westerly wind, 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 water surface temperature. For their studies they excluded all data with these conditions. This would lead to data gaps in the time series of water surface temperature during westerly (offshore) wind conditions, meaning that no water surface temperature data would be available for the timesteps where it is actually needed to estimate evaporation from the water surface, as the station measures evaporation from the land surface for offshore wind.

– 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 strongly.

– Because of the aforementioned problems with satellite data we followed the advice of another paper from Nehorai et al. (2013), which shows that "SST is highly correlated to air temperature ($R^2 = 0.93 - 0.98$) in all seasons". Based on these results the similarity approach was used to calculate water surface temperature from air temperature.

– Of course there is dependence in the data, but we also checked the results of the similarity approach with a short term experiment of about 5 days, where longwave and shortwave radiation was measured directly over the water surface. From the outgoing longwave radiation radiation temperature of the water surface was calculated and compared to the calculated Tw using the similarity approach. A correlation of 0.8 was achieved. This is quite good, considering the uncertainty of the radiation measurements, as the radiation sensor was getting covered with salt through the spray over the course of the 5 day experiment.

For offshore winds, the source area is partly over water, partly over land. The ΔT estimation should be a combination of ground surface temperature and water surface temperature. There again, an estimation of the ground surface temperature (perhaps after some assumptions of the emissivity) would be appropriate. You could perhaps also use the upward longwave radiation flux measured at the neighbouring ground station.

For the multiple linear regression approach, data from offshore winds are excluded. So land surface temperature is not needed. As the regression model is used to calculate the evaporation from the water surface for offshore wind conditions, the water surface temperature is needed and can be calculated with the MO Theory.

- *For offshore winds, I do not know any mean to deduce E_{surf} , unless you make assumptions on the water vapour at the ground surface. You could use the similarity profile, but meanwhile you would make the choice of a model and Sections 3.3 and 4.4 would become useless. For onshore winds, $E_{surf} = 0.65 \cdot Ew(Tw)$ could be an appropriate estimation, Tw being the satellite sea surface temperature, instead of Tmo. A sensitivity study of the regressions to the error in Tw (which determines ew) could also be informative.*

  This paper focuses only on the energy balance of the water surface. The energy balance of the land surface is not discussed and beyond the scope of this paper. For investigating the energy balance of the land surface we have a different station which is further inland and therefore not influenced by the water. Therefore there is no need to deduce E_{surf} from the data set.

C3) If we consider the 3 objectives you propose to fulfill : i) is fulfilled by the EC method, but you do not need to use any multiple regression model to quantify the offshore conditions : you directly measure them. The way you can link these flux measurements with local air or surface parameters is another issue.
Yes the offshore conditions are measured but not the fluxes from the water surface for offshore conditions. For offshore wind conditions the source area of the measured flux is the land surface. As the aim is to get the full diurnal cycle of the flux from the water surface (which would be measured if the station would be located on a raft), it is necessary to apply the multiple regression model and estimate the flux values for offshore wind conditions.

ii) I think you cannot totally achieve this aim since you can only access to the local terms of the energy budget and make assumptions on the terms you have to neglect.

We fully agree. We can only get the local evaporation at the measurement site and not the evaporation for the whole lake. This point will be rephrased that it will become clear that these are local values.

iii) OK if you define what 'evaporation' is. You could also add that you want to assess the capacity of these models to retrieve the 'evaporation' term, in the future, when the EC sensors are not available any more (you suggest this in the conclusion).
we will add your suggestions to point iii.

3 Other Remarks

Thank you very much for the detailed and helpful remarks. First, we will provide answers to the questions raised by the reviewer. Afterwards we list the comments about readability and linguistical problems. We will consider all of these comments in our revised version of the paper and will revise the text accordingly.

3.1 Questions

13 - p2, line 7 : during which period this decrease happened ? And 60-400 denotes a very large variability. Can you explain why ? (variability among the authors ?)

The decrease was caused by the construction of dams and canals along the Yarmouk river and Lake Tiberias/Kinneret mainly between 1955 and 1964. Since then only about 10% of the natural discharge of the Jordan river enters the Dead Sea. The variability of the inflow results from variability among the authors.

17 - p2, line 16 : ←'shifting of the fresh/saline groundwater interface' ('of the' has been added). Please define sinkholes, I did not know this phenomenon.

Sinkholes are holes or depressions in the ground formed by subsurface erosion or removal of soluble bedrock and the collapse of the surface layer.

18a - p2, line 25 : why westerly winds would be harmful, compared with easterly winds ? You could also delete 'the' at the end of the line, before '1940'. I suppose this harm comes from the fact that easterlies carry drier (continental) air, whereas westerlies carry moister air. But this cannot be guessed from what you wrote here.

Thee westerly winds have often high wind velocities enhancing the evaporation and thus accelerating the lake level decline.

29 - p5, line 5 : is it a tipping bucket rain gauge ?

Yes, it is a tipping bucket rain gauge.

34 - p5, eq (2) and line 24 : usually, $LE = \rho_a Lv\overline{w'r'}$ *or* $LE = \rho_a Lv\overline{w'q'}$ *where r and q are the water vapour mixing ratio and specific humidity, respectively. Brutsaert (which you refer to in the following) uses the latter definition for the evaporation rate :* $Ev = \rho_a\overline{w'q'}$. *In addition, if I remember well, the hygrometer converts the absolute humidity in water vapour mixing ratio, using T=20°C and P=1013.25 hPa. Perhaps you could consider using r or q instead of a.*

We can of course change eq(2) to the more common form with specific humidity. In our calculations of the turbulent fluxes with the EC software we use absolute humidity as this is the raw output of the IRGASON.

39- p6 or before (p5) : did you use a constant calibration coefficient for the IRGASON (when was the calibration done ?) or did you calibrate the hygrometer measurements against the low frequency humidity *measurements ?*

The IRGASON was calibrated before the experiment but not in between, so constant caplibration coefficients were used.

41- p6, line 29 : 'when the variability of the signal' . Could you define the variability (standard deviation/average ?) ? I think it is 0.6 and not 0.6%

This means that if 10 min averaged normalized signal strength varied from one time step to the next more than 0.006, which is 0.6%, the corresponding 30 min flux value was excluded. This procedure was introduced due to the fact, that we found an increasing variation of the signal strength with the decrease of the total signal strength. Most likely due to contamination of the glass window of the instrument.

46 - p7, subsection 3.2 : once you have made the corrections indicated in point 2, I suggest that you keep on calculating the multiple regressions, but not with the aim of parameterizing the offshore conditions. Rather, to show how the classical relationship between the latent heat flux and the wind and/or vapour pressure deficit behaves, under the specific conditions of a semi-arid area that is influenced by sea breeze, slope breeze or both. The multiple regressions should be done for onshore and offshore data separately (provided that 19% of the dataset is enough to apply your method to the offshore data). Note that the regressions you examine are similar to the aerodynamic (or bulk model) from Brutsaert. That is why I am not surprised by the good correlations you obtained in Table 4 for V0. That is also the reason why I do not agree with the idea of using these multiple regressions to calculate offshore winds. I also think that the presentation of the simple regressions should be shorter since they are known to fail to represent the flux but they can serve as a base to compare the multiple regression $H = f(U;\Delta T)$, to the regression $H = f(U\ \Delta\Theta)$, and the same with LE, U and Δe. Please also think of using $\Delta\Theta$ instead of ΔT, although the difference will be very small. You may have noticed that I added a multiple regression $H((U;\Delta T)$. The reason is because the BREB method is partly based on this correlation.

We think that the reviewer suggested the additional multiple regression for offshore data based on its wrong impression of our work flow at the beginning (also discussed in C1+C2). As we already clarified in our answer to C1+C2 that we do not use the offshore data for any calculations, there is, in our opinion, no need to establish a regression model for this part of the data.
The second point of the reviewer was to shorten the results of the simple regression results (P.13 l.1-6) and add a comparison of the multiple regression with the simple regressions. We will go over the paragraph and will add the following conlcusion to it: it can be seen from the comparison that wind speed has a much stronger impact on evaporation that the vapour pressure deficit (VDP) in spring, summer and winter, but that in autumn the VDP has an considerable impact as well.
The third point raised in this comment was a regression for the sensible heat flux with different variables. This was already done during the analysis. The multiple regression of H=f(U,ΔT) achieved a correlation of 0.93. These results were not presented in the paper as we wanted to keep the focus on the latent heat flux.

50 - p8 : I would substitute 'aerodynamic method' for 'aerodynamic or mass transfer method'. In fact it is the bulk method, frequently used to estimate surface fluxes over the sea, where the EC method or dissipative method are not easy to implement. Brutsaert (1982) refers to it as the 'bulk transfer' method and it is based on similarity profiles assumptions and the relationship between fluxes and wind or scalar gradients (through the Dalton or Stanton numbers). According to Brutsaert (p88 in my edition, reprinted in 1984)

$$Ev = C_e \rho_a v_a (q_{surf} - q_a) = C_e \rho_a v_a (e_{surf} - e_a) \frac{0.622}{p} \tag{1}$$

Without telling it, you assume equal transfer coefficients for evaporation and momentum ($C_e = C_d$). C_e is a mass transfer coefficient for evaporation and C_d is the drag coefficient $= \frac{u_*^2}{v_a^2}$. Introducing the logarithmic wind velocity gradient under neutral conditions, which is a second assumption, Ev becomes :

$$Ev = \frac{k^2}{(ln\frac{Z}{zo})}^2 \rho_a v_a (e_{surf} - e_a)\frac{0.622}{p} \quad (2)$$

So K_E you identify in Table 1 should not contain ρ_w. I suppose you needed to add it since the kinematic flux you calculated is in term of absolute humidity (but you should have divided Ev by ρ_a and not ρ_w).
To conclude, I suggest you add a remark concerning the assumption that Ce = Cd. Perhaps this could be explained in an additional Appendix (3). I also suggest that you move into the present subsection, your sentence from p17, lines 12-13 : 'the aerodynamic approach is the only approach designed for sub-daily time intervals'. And you could add '(typically 30 min in this study)'. I find that you well address this timescale issue in the following, specifically with Table 6 where you show the results. However, it should be also clearly mentioned in subsection 3.3 (for the 4 models).

Thank you very much for this comment. We will follow your suggestions and add an explanation about the assumptions and steps we made, including Ce=Cd, and the used of the logarithmic wind profile. We will also address the issue that only the aerodynamic approach can be used for sub-daily calculations and correct the text accordingly.

57 - The hysteresis model from Duan and Bastiaanssen, $\Delta Q = aRn + b + c\frac{dRn}{dt}$ should be described including the discussion about the term $\frac{dRn}{dt}$. Note also the dependance of c on the range and variability of the water surface temperature. V3 is a specific case of V2 where b=0 and c=0, a being obtained as 'the deviation of the default version from the measurements'. Did you try to determine specific (a,b,c) for your own dataset, just to quantify the deviation relative to Duan and Bastiaanssen's results ? I do not suggest to include them in the models you use, since, doing so, you would invalidate the V5 regressions.

Thank you for this comment. We will add some additional explanation about the hysteresis model to the paper. We will also explain that in our analysis we first used the proposed ansatz from Duan and Bastiaanssen $\Delta Q = aRn + b + c\frac{dRn}{dt}$, and calculated specific a,b,c from our data set. This was done, as Duan and Bastiaanssen stated in their paper that 'a,b, and c vary largely among lakes. [...] and that lake-specific coefficients should be determined'. Then we investigated the special case b=0 and c=0. V5 is an additional 'special' case were we repeated the calculations using the hysteresis model and the coefficients from Kohler and Parmele, which means V2 and V5 are not the same.

58 - Could you please be careful to discuss the assumptions of the other two models and give additional information for Kohler and Parmele's work ? I would not say that Ts has been removed (if I understood correctly) but that it has been estimated from the long-wave radiation flux. This is no-doubt an improvement, relative to your initial estimation from the similarity profile. Please use the same symbol to design the water temperature as the one you have used in the previous section, unless you want to distinguish it on purpose.
Thank you for the comment. We will revise the description of the sensitivity studies that it will become clearer to the reader. Kohler and Parmele did not estimate the surface temperature from longwave radiation but removed T_s by doing following approximation. He used the first two terms of a binominal expansion

$$L\uparrow = \epsilon\sigma T_s^4 = \epsilon\sigma(T_a^4 + 4T_a^3(T_s - T_a)) \quad (3)$$

and then used $T_s - T_a = (E_s - e_a)/\Delta$ to derive following parameters

$$L' = L\downarrow - L\uparrow = \epsilon_w \cdot L\downarrow - \epsilon_w \cdot \sigma \cdot T_a^4 \quad (4)$$

$$\gamma' = \gamma + \frac{4 \cdot \epsilon_w \cdot \sigma \cdot T_a^3}{K_E \cdot \rho_w \cdot L_v \cdot v_a}. \quad (5)$$

62 - Fig. 2 : Please add 'prec' after 'daily precipitation amount' in the caption. Which temperature is Ta : ultrasonic at 6m, BetaTherm probe at 6m, HC2S3 probe at 2m? Please represent T_{surf} instead of Tmo. You could also show $T_{surf} - T_a$. Please represent $e_{surf} - e_a$, and perhaps also qa. I suppose the air is very dry during summer. It would be interesting to show the annual evolution of the air specific humidity. 200 Wm^{-2} are enough for the H vertical axis. It would be convenient to add a thin line for 0 Wm^{-2} in the lower panel. Did you try to represent the daily average parameters (Rn, H, LE, ΔQ) on the same graph (with a *more appropriate scale on the y-axis), to be able to see the phase shift between the annual maxima and also whether the Rn variation relative to the time is linked or not with ΔQ (in relation with the hysteresis model from Duan and Bastiannssen).*

Ta is 2m temperature. The use of T_{MO} and not T_{surf} was discussed in C2. As explained, there is no way to determine T_{surf} from satellite data and therefore we will keep T_{MO} and e_{MO} in the graph, but we will try to present the daily averages in the graph as suggested from the reviewer. If the graph is still well readable we will change the graph in the revised version.

65 - p10, line 6 : 'the annual precipitation normal of 80 mm' : the word normal is not accurate. What is the period considered by Goldreich, 2003 ?
The period which was considered by Goldreich was the climatological standard normal period 1961-1990.

68 - p10, lines 14 and 15 : are 32 and 26 % relative to lake breeze or lake breeze+ synoptic conditions ?
These values are relative to the total amount of days in winter.

76 - p10, discussion on the energy budget : you assume that the energy budget is closed and you never discuss the frequent non-closure energy budget problem that is reported in several studies in the literature (see Foken et al. in Aubinet et al. p108-109). You cannot avoid this discussion, even if there is no mean to estimate the error, especially because your measurements are done at the boundary between the marine and the continental surface layers. Under these conditions, the surface change may generate large scale heterogeneities that are unlikely to be correctly taken into account by the local measurements. You show in Fig. 3, extreme values of ΔQ that are of the order of the net radiative flux in spring and summer. It is unlikely to be true. Anyway, it is known that the shorter the timescale, the larger the non-closure. By contrast, the average ΔQ (daily average) is about 100 Wm^{-2} during summer and decreases down to a few tenth of Wm^{-2} in winter, which are rational values (I remember that LE has to be recalculated, but it should not be very different).
Thank you for the comment. We agree with the reviewer that for every EC system there are uncertainties and a possible non closure of the energy balance. The reviewer is right, that we should address this point in context of the calculation of the heat storage as a residuum. So we will address this point in the context of section 4.1.

3.2 Linguistical Comments

Thank you very much for these detailed comments. We will consider all of these comments in our revised version of the paper and will revise the text accordingly.

5 - The term 'evaporation' you frequently use is not accurate enough. I will point it out in the following.
6 - p1, line 6 : 'total annual amount measured' →'total annual amount of evaporation measured' (in this case, you do not need to be more accurate since you provide the evaporation unit).
7 - p1, line 7 and further on : 'vapour pressure deficit' ←'water vapour pressure deficit'
8 - p1, line 8 : 'Consequently' is not appropriate. Perhaps 'in fact' could be used instead. What do you mean by 'evaporation amounts' ?
9 - p1, line 10 could be changed to ←'during daytime. During nighttime, evaporation rates are also larger than the daytime evaporation rates, due to strong ...'. Why do you use 'evaporation rate' this time ?
Note that this result will perhaps require corrections, in light of what is said in the following (see my final remarks for instance).
10 - p1, line 11 : The link 'Furthermore' is somewhat awkward. You should explain here why you calculated the regressions. By the way, I think that the multiple regressions should be established for another purpose (see my remark 46-).
11 - p1, line 14 is clear and nice. I skip lines 15, p1 to the Introduction, p2.
12 - p2, line 6 : you could add 'down' after '90%'.
14 - p2, line 9 : 'The total amount is about' ←'The total amount of loss is about'
15 - You could add a budget equation such as : 10^6 (400+240-250)+evaporation = -650 10^6, which gives

evaporation = -1060 $10^6 m^3 a^{-1}$, which is in the range 700 - 1400 10^6, indicated by Gavrieli et al., 2006.
16 - p2, lines 13-14-15 : I would replace ' Evaporation is not only ....environmental problems.' by something like 'It is important to assess the water budget components of the Dead Sea for a climatological purpose, but it is also a priority for the people and the socio-economic development of the region to anticipate the evolution of these components and the consequence for the environment. For instance, the lake level decline causes severe environmental problems . ' (you may of course change the words).
18b - p 2, line 29 (no link with 18a) : I do not understand 'especially' in this context ('In addition' ?). Perhaps you could also mention whether it is a fresh water fish. Ein Feshkha reserve ?
18c - p3, line 5 : I would add 'in the evaporation estimations' just before '(Stanhill, 1994'.
18d - p3, lines 6-7 : I would remove : 'Furthermore, the governing factors of the Dead Sea evaporation, e.g. wind velocity, vapour pressure deficit, or net radiation, have to be identified, to validate the indirect methods'. These parameters are governing factors every where in the world and you do not have to prove it. I think this sentence is confusing at this point.
18e - p3, line 8 : 'with a high temporal resolution' instead of ',in high temporal resolution'.
19 - p3, line 10 : 'continues' ← 'continuous'
20 - p3, line 12 : according to Wikipedia, it seems that Lake Kinneret is the same as Lake Tiberias mentioned by Kottmeier et al. (2016) (it is only a remark, you choose the name you prefer). You could perhaps add that it is crossed by the Jordan river which partly feeds the Dead Sea. It is not a major piece of information but I find it nice to provide the reader an idea of the geographical environment.
21 - p3, line 13 : 'to the authors knowledge' instead of 'as to the authors knowledge'.
22 - p3, line 14 : 'Therefore, long-term eddy covariance measurements are conducted' ← 'That is why, in the frame of the international DESERVE project (Kottmeier et al., 2016), long-term eddy covariance measurements were conducted'
23 - p3, line 17 : 'provided' and 'was' instead of 'provides' and 'is'
24 - p3, lines 17-19 : I perhaps misunderstood but it seems to me you did not use the data from these stations. Is it useful to quote them ?
25 - p3, lines 19-21 : 'Provide', 'Evaluate' and 'Evaluate' instead of the same with 'ing'.
26 - p3, Measurement site : it is difficult to distinguish in Fig. 1, how far the Judean mountains highest submits are from the lake and what their height is (hills or mountains ?). The Moab mountains are clearly shown.
27 - Fig. 1 : you forgot to write 'Jordan river' in panel (a). Landsat with an L in the caption. The red arrows are a little confusing in panel (b) : simple lines instead of arrows would be enough.
28 - p5, line 4 : Rotronic
30 - p5, line 7 : please add 'open path' before 'integrated gas analyser'. I suppose that at 6m, it did not suffer from spray, even under strong onshore wind conditions ...
31 - p5, line 9 : 'From the 20 Hz data evaporation was calculated using the eddy covariance technique' → 'The latent heat flux was calculated from the 20 Hz data using the eddy covariance method.'
32a - p5, line 9-10 : ← 'The principle of the method, the post-processing and data quality control steps are presented in Sec. 3.1'.
32b - p5, lines 11-12 : please consider the multiple regressions again (after 46-)
33 - p5, eq (1) and line 23 : the ultrasonic anemometer provides the virtual air temperature and not the air temperature Ta. The Schotanus correction is made, as you say in Sec. 3.1.1, to take this point into account.
35 - p5, eq (3) and line 27 : as said before, Tw should be Ta.
36 - p5-6, lines 27 to 5 : the text should be deleted from 'For salt water ..' to the end of the subsection.
37 - p6, line 8 : 'measurement limitations' you could add 'of the sensors'
38- p6, lines 18 to 24 : it seems to me that the order should be : spectral corrections, Schotanus correction and Webb correction unless you applied an iterative process.
40- p6, line 28 : 0.5 g/kg ?
42 - p6 line 31 to p7 line 2 : you say too much or not enough. It would be nice to describe the tests and to say what ITC is.
43 - p7 and further on : I would replace 'fetch' by 'source area'.
44 - p7, lines 6-7 : please consider again the rejection of these data : I agree that it is important to distinguish them from the onshore measurements data, but the fluxes are what they are and do not have to be rejected. Nevertheless, it is important to quantify this contribution since the source area may be different.

45 - p7, lines 7 to 9 : I would replace the 2 sentences 'For southerly wind directions ... 600m away from the headland.' by 'For southerly and northerly wind directions the source area is over water and the average source area contributing to 80% of the flux ranges from 0 to 300 m and 0 to 600 m, respectively, ahead of EBS.'
47 - p8, line 1 : 'Indirect methods to estimate evaporation' $\leftarrow$'Description of four indirect methods to estimate evaporation'
48a - p8 : I would move the first two (essential) sentences of subsection 4.4 ('For the calculation of evaporation -please insert a comma here-, several equations, based on .... at least 7 days') to the beginning of section 3.3.
48b - p8, line 5 'an overview of which sensitivity study is performed' (I added 'of').
49 - Ev is not defined. I suppose it is the evaporation rate defined as Ev=ρ_a w0q0 (Brutsaert, 1982).
51 - The sensitivity study, referred to as V1, is performed to address the stability issue. The presentation of the stability factors you refer to (Cline 1977) p. 17 should also be moved to the present subsection. I would also describe the new KE (including the stability factor) in Appendix 3. You could also add that the stability functions for wind and heat are expressed in terms of the bulk Richardson number, which allows estimating the stability when the turbulent fluxes are not known.
52 - Energy budget : Here again, ρ_w should vanish if you use the specific humidity instead of the absolute.
53 - I suggest that you write down the budget equation as Giadrossssich, 2015 did (their eq 1) : Rn + Anet = LE + H +ΔQ , where Anet is the net heat advected into the lake (by stream flow and precipitation minus the heat loss due to evaporation minus the heat transferred at the bottom of the lake) and ΔQ is the heat storage per unit area in the lake (for most cases) or in the ground (for specific cases with strong offshore winds. Under these conditions, Anet can be ignored). This energy balance applies to timescales larger than the day due to the advection term that cannot be known at a short timescale.
54 - With V0, you neglect ΔQ and Anet . [Note that the resulting reduced budget equation can also be applied at a sub-daily scale.] Neglecting ΔQ is a coarse assumption that is valid only under specific and occasional conditions. I think you should mention it at first and say that V0, even unrealistic, is a basis for the BREB method that will be improved by V1 and V2. I mean that V1 and V2 should not only be considered as sensitivity studies for V0, but as 2 alternative methods for V0.
55 - p9, line 8 : you forgot to mention the water vapour deficit.
56 - β, the Bowen ratio should be defined as H/LE. When fluxes are unknown, β can be approximated by the expression you give, provided that K_Θ, the Stanton number for temperature = Ce, the equivalent for evaporation. Also, be careful not to use the same symbol for the Bowen ratio and the activity of water in Appendix A (I would keep β for the Bowen ratio).
59 - Table 1 : caption : default versions (V0) in Sec. 3.3 and 4.4. (just add 3.3) Fn and Δt are not used and ρ_w should not be used. Please take into account my remarks in 2- Priestley-Taylor is presented as the 3rd method in Sec 3.3.
60 - Meteorological conditions : this subsection will have to be read again after new LE, T_{surf}, Δe values ...
61 - For some parameters, the daily values and their evolution are also interesting to discuss in addition to the extreme values.
63 - p10, line 4 : 'long term annual mean' : during which period ?
64 - p10, line 5 : please add 'sometimes' before 'exceeded' 66 - p10, line 7 : 'made' instead of 'makes'
67 - p10, line 9 : 'only during the winter seasons a different behaviour was found' $\leftarrow$except during winter, when the wind increased in connection with the convective activity, a different behaviour was found'
68 - p10, lines 14 and 15 : are 32 and 26 % relative to lake breeze or lake breeze+ synoptic conditions ?
69 - p10, line 15 : Please indicate the direction of the downslope breeze (north-westerly)
70 - p10, line 16 : 'yielded' instead of 'lead to'
71 - p10, lines 18, 19, 24 : 'exceeding' instead of 'of over'
72 - p10, line 22 : 'November a Red Sea Trough with a central axis advected dry and warm air'$\rightarrow$ 'November, when a Red Sea Trough advected dry and warm air'
73 - p10, line 25 : 'However, at' $\leftarrow$'However, on'
74 - p10, line 26 : 'in winter latent heat flux values' $\leftarrow$'in winter some latent heat flux values'
75 - p10, line 27 : the energy balance equation should have been shown in subsection 3.3. You only need to say that at this timescale (24h), Anet is ignored. Do you think it is still correct after rainfall ?
77 - p10, line 31 : please reword 'used for heating the lake, which is stronger in spring than in winter'. This

is grammatically false.
78 - p10, line 33 : I do not see that ΔQ is negative in winter.
79 - p 12, fig 3 : it is usually required to indicate the delay between the UTC and local time. You can keep LT and indicate, on the first time (UTC +3h). It would be also interesting to add in the caption the approximate time of sunrise and sunset, especially from Spring to Autumn.
80 - p12, line 4 : ←'and, thus, [at] most of the [days] data within this time frame' (remove the [words]).
81 - p12, line 6 : ←'also for the study of the intra-annual .... this gap' (instead of these gaps).
82 - p12, line 6 : 'A multiple regression model was applied ... for offshore conditions' : As said before, I do not agree with this method : I'm waiting for your decision, regarding the suggestions I made in 2-

References

Nehorai, R., Lensky, I. M., Lensky, N. G., and Shiff, S. (2009). Remote sensing of the Dead Sea surface temperature. J. Geophys. Res. Oceans, 114(C5). C05021.

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