

# 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.

# 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.

1 - 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.  $3148.4-2.37 T_a$  and not  $Tw$  as you use in Eq. 3.  $T_a$  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). 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].

2 - 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, I 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. I 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, I 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). For offshore winds, the source area is partly over water, partly over land. The  $\Delta T$  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 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,  $E_{surf} = 0.65 Ew(Tw)$

- could be an appropriate estimation,  $T_w$  being the satellite sea surface temperature, instead of  $T_{mo}$ .
- A sensitivity study of the regressions to the error in  $T_w$  (which determines  $ew$ ) could also be informative.

3 - 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. 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. 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).

4 - In general, you could perhaps take more benefit from the literature you quote. For instance, you mention Giadrossich et al.'s work for minor reasons whereas you could compare their results to yours, since the measurements they carried on, are characteristic of a semi-arid area, like the area you study.

I think modifications explained in 1 to 4 should be done before I read the paper again. However, I add below more accurate remarks and some comments, that could be useful to improve the paper (at least the beginning).

### 3 Other remarks

[Note : there is no intention in numbering e.g. 18a, 18b, 18c : I simply inserted this lettering because I wanted to add remarks without changing the numbers ...].

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%'.

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 ?)

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).

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.

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.

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

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

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  $T_a$ . The Schotanus correction is made, as you say in Sec. 3.1.1, to take this point into account.

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  $T=20^\circ\text{C}$  and  $P=1013.25\text{ hPa}$ . Perhaps you could consider using  $r$  or  $q$  instead of  $a$ .

35 - p5, eq (3) and line 27 : as said before,  $T_w$  should be  $T_a$ .

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.

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?

40- p6, line 28 : 0.5 g/kg?

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%

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.'

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  $\Delta e$ . Please also think of using  $\Delta \theta$  instead of  $\Delta T$ , 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.

47 - p8, line 1 : 'Indirect methods to estimate evaporation'  $\longrightarrow$  '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  $\overline{Ev} = \rho_a \overline{w'q'}$  (Brutsaert, 1982).

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)

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

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,  $\overline{Ev}$  becomes :

$$Ev = \frac{k^2}{(\ln \frac{z}{z_0})^2} \rho_a v_a (e_{surf} - e_a) \frac{0.622}{p}$$

So  $K_E$  you identify in Table 1 should not contain  $\rho_w$ . 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  $\rho_a$  and not  $\rho_w$ ). To conclude, I suggest you add a remark concerning the assumption that  $C_e = C_d$ . 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).

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  $K_E$  (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,  $\rho_w$  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) :  $Rn + A_{net} = LE + H + \Delta Q$ , where  $A_{net}$  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  $\Delta Q$  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,  $A_{net}$  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  $\Delta Q$  and  $A_{net}$ . [Note that the resulting reduced budget equation can also be applied at a sub-daily scale.] Neglecting  $\Delta Q$  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 -  $\beta$ , the Bowen ratio should be defined as  $H/LE$ . When fluxes are unknown,  $\beta$  can be approximated by the expression you give, provided that  $K_\theta$ , the Stanton number for temperature =  $C_e$ , 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  $\beta$  for the Bowen ratio).

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.

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  $T_s$  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.

59 - Table 1 : caption : default versions (V0) in Sec. 3.3 and 4.4. (just add 3.3)

$F_n$  and  $\Delta t$  are not used and  $\rho_w$  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,  $T_{surf}$ ,  $\Delta_e$  values ...

61 - For some parameters, the daily values and their evolution are also interesting to discuss in addition to the extreme values.

62 - Fig. 2 : Please add 'prec' after 'daily precipitation amount' in the caption. Which temperature is  $T_a$  : ultrasonic at 6m, BetaTherm probe at 6m, HC2S3 probe at 2m? Please represent  $T_{surf}$  instead of  $T_{mo}$ . You could also show  $T_{surf} - T_a$ . Please represent  $e_{surf} - e_a$ , and perhaps also  $q_a$ . I suppose the air is very dry during summer. It would be interesting to show the annual evolution of the air specific humidity.  $200 \text{ Wm}^{-2}$  are enough for the H vertical axis. It would be convenient to add a thin line for  $0 \text{ Wm}^{-2}$  in the lower panel.

Did you try to represent the daily average parameters (Rn, H, LE,  $\Delta Q$ ) 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  $\Delta Q$  (in relation with the hysteresis model from Duan and Bastiaanssen).

63 - p10, line 4 : 'long term annual mean' : during which period?

64 - p10, line 5 : please add 'sometimes' before 'exceeded'

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 ?

66 - p10, line 7 : 'made' instead of 'makes'

67 - p10, line 9 : 'only during the winter seasons a different behaviour was found' → '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' → 'November, when a Red Sea Trough advected dry and warm air'

73 - p10, line 25 : 'However, at' → 'However, on'

74 - p10, line 26 : 'in winter latent heat flux values' → '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),  $A_{net}$  is ignored. Do you think it is still correct after rainfall ?

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  $\Delta Q$  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  $\Delta Q$  (daily average) is about  $100 \text{ Wm}^{-2}$  during summer and decreases down to a few tenth of  $\text{Wm}^{-2}$  in winter, which are rational values (I remember that LE has to be recalculated, but it should not be very different).

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  $\Delta Q$  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-

I'm convinced that the comparison of the 4 models is very interesting, but I need to see results that I can rely on. Specifically, I do not trust the results you show in Fig. 4. However, I'm ready to recognize that I am wrong.

I stopped the review here.