HESSD

Interactive
comment

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 #2

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General

This a very interesting paper, addressing an important environmental issue (evaporation from the Dead Sea), presenting very important data and (apparently for the first time) directly measured evaporation rates and thus adding important new information to our knowledge. There is no doubt, therefore, that the paper should eventually be published in one or the other form. There are three main pillars: first, to measure, directly, the lake's evaporation using an EC station on the shore (therewith employing the footprint of the water surface). This provides direct measurements for about 70% of the time. I think this approach is well motivated, well explained and also 'well executed'

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(all the necessary data treatment, corrections, QC, etc.).

To estimate, as a second pillar, evaporation during the remaining 30% of the time, a statistical model is trained using the onshore wind conditions and the available information during these conditions, to estimate lake evaporation during offshore wind direction. This is very appropriate (and possibly novel) as an approach, and so is the statistical approach, its presentation (and results). However, it has the drawback that the estimated evaporation rates are among the largest during the whole year and thus contribute a substantial fraction of the total (yearly, monthly, daily – the shorter the more variable) evaporation. Therefore, the statistical model does not only have to be tested using the 'usual' tests (cross-validation, etc.) based on the available onshore conditions (what is convincingly being done), but attempts should be made to support the hypothesis that the same statistical model applies (yields the claimed 'good' statistics) when the input data stem from offshore situations (high wind speed in combination with low water vapor pressure deficit). Some suggestions are provided in major comments 2 and 3.

As a third pillar, empirical estimates (for evaporation) are compared in their performance to the measured evaporation. This is adding a great deal of value to the paper. Unfortunately, for three out of four methods the comparison is not valid (because net radiation is not measured over water – which basically invalidates all the estimates; see major comment 1). Furthermore, the estimation of heat storage (in the water) is also flawed (see again major comment 1), so that all the different 'versions' tested are not really conclusive (with the exception of V1). Indeed, the results show that all the empirical methods using net radiation give results quite different from the observations (if only measured – and not estimated – values would be considered, this finding would probably be even more pronounced). The only 'reliable' empirical method is the aerodynamic approach (not using Rn). The problem with the different fields of view (for radiation and turbulent fluxes) could somehow be overcome (for example by using satellite – or other – observations [even literature values would be better than nothing]

to correct for albedo differences between land and water, and by using the [simultaneously measured] land surface temperature [from the two additional EC sites over land] and the estimated lake surface temperature to correct for different longwave outgoing radiation). To actually estimate heat storage in the water from the available data I consider virtually impossible – so that the hysteresis model is possibly the only available source.

Overall, the addressed issues call for truly major revisions before this paper can e published in HESS.

Major comments

1) Heat storage is calculated as the residuum (P10, l. 26): the authors use the same notation (delta_Q) as above for the 'heat storage of the lake' (p9, l. 5). So, is this meant to yield the heat storage of the lake using the local energy balance? This is not appropriate for two reasons. First of all, the energy balance is based on the turbulent fluxes (which can, in my opinion, be interpreted as reflecting the heat fluxes in the footprint, i.e. over water – of course, if the wind direction is accordingly). Net radiation, however, has a much smaller 'field of view' (a circle with a radius of maybe 2 m a for measurement height of 6 m), so that the albedo is that of the land surface, and the same is true for the longwave outgoing radiation. The considered energy balance, therefore, is reflecting (a combination of) two different surfaces – and the difference cannot be attributed to 'anything' (if not a careful disentangling of the differences between radiation conditions is performed). Second, even if the various sensors would see the same surface, the energy balance (measured at 6 m agl) cannot be expected to be closed. There will be mean advection (possibly even in the vertical – as this location is so close to a step change in surface conditions; water – land), storage (in the air layer between the surface and the sensors!), possibly even vertical flux divergence. Finally, for the local energy balance (still assuming that all the observations correspond to the same surface type), one would also need the ground heat flux. Give the importance of this term (P10, l. 30) the authors need definitely to do something about it.

2) Statistical model to estimate latent heat flux during offshore conditions: (P14, l. 8) Values up to 200 Wm-2...: Interestingly, the estimated values are larger than the measured values (even on average!). Given the statistical model and the high wind speeds especially during the evening hours – in combination with presumed small water vapor pressure deficit - in spring and summer (Fig. 3), this suggests that the statistical model has possibly been used outside the conditions, for which it has been 'constructed'. In other words, the statistical model is trained for cases of high wind speeds (but possibly not even as high as the downslope winds) in combination with (relatively) small water vapor pressure deficit while it is being used for high wind speed and large delta_e. Since these estimated values are not only to fill some gaps in the measured time series but produce the largest values for characteristic times (i.e., after the evening transition), the authors should try to make a very strong case for these estimated latent heat fluxes. In this sense, the statistical model should not only be evaluated in the 'classical sense' (as it is being done – and very convincingly!), but also the question (hypothesis) should be addressed, whether the onshore (training) and offshore (application) conditions are comparable. In other words, how is delta_e over land related to delta_e over water? For this, potentially the two additional EC sites (p3, l. 17 - they are apparently available but not used in this study) could be employed. Similarly, the question should be addressed how the strong downslope winds are related to typical wind speed over water. The question here is therefore whether there is any suitable information (possibly from other studies or sources), which would support the hypothesis that the observed (offshore) wind speed can be used to estimate the wind speed over the [entire] Dead Sea.

3) The Discussion Section as a whole is, first of all, more a summary than a discussion. Much of what has been stated before is repeated (and the 'discussion' consists to some extent in adding some literature values). The statistical model, for example, is repeated to be good enough (no discussion), rather than addressing potential difficulties (see major comment 2). The 'problem' with the downslope winds (having a stronger wind speed that over the lake, and (probably much) larger water vapor pres-

sure deficit is mentioned - but only mentioned to yield a 'slight overestimation'. Based on what is this called 'slight'? Is it 10% (and would 10% be slight)? Or 50%? (but only occurring during 30% of the hours? – and what is then slight?). I think this would be a discussion. Another point that apparently needs discussion is the fact that the radiation measurement does not 'see' the same surface as the authors want to probe with their EC system, i.e. the lake surface. In fact, I think that either all the aspects, which include Rn have either to be removed from the paper, or an estimate has to be made to establish a method to estimate Rn over water from measured Rn over land (see major comment 1). The discussion then, would consist of the associated uncertainty and the potential impact on the interpretation of the empirical methods (i.e. their performance). Given the relatively large uncertainty in the statistical model, a useful contribution to the discussion would be to test the empirical relations for only a subset of days (for the 1 day averaging period, say), for which the impact of the statistical regression model is minimal (only a few or no estimated evaporation hours, mostly measured values). If then the comparison to the 4 empirical methods (Tab 6) would be robust, this would indeed be an indication that the conclusions regarding the appropriateness of the empirical relations are supported by data (not the statistical model). This last discussion, of course, would only make sense if the 'radiation problem' had somehow been overcome. Finally, an important point for the discussion seems to be that the empirical estimates are relatively good 'average estimates' (28 days) – but do only have reduced predictive (diagnostic, that is) skill for short time scales.

Minor comments

P5, l.4 I don't think I have ever heard of a Rototronic sensor

P5, l. 4 the height of measurement is not given for the radiation, precipitation and pressure sensors. Please specify.

P5, l. 25 I agree with the present authors (in contrast to the other reviewer) that the appropriate conversion should be based on the water temperature and not the air tem-

perature (the differences will be small, though). <w'a'> is already an energy flux (i.e., the kinematic flux of absolute humidity), which is only converted into energy units by multiplication with L_v. The corresponding energy (enthalpy) is calculated at the location where the process of evaporation takes place and the relevant temperature is the lake surface temperature (whether this is called T_w, the very top of the water or T_s, the very bottom of the air, is the same). I don't think that the 'constancy of the fluxes' in the SL is a valid argument for using the air temperature (at 6 m agl in the present case) since the fluxes are only 'constant' within the SL, but not below (note that the SL has also a lower boundary, i.e. the laminar layer with a thickness of some millimeters over water wherein the turbulent fluxes are zero by definition – and rapidly change to their 'atmospheric surface value' at its top). The 'SL theory' (including the constant flux assumption) produces a temperature profile (e.g., eq. 11.12 in the text book of Arya (1988)), which is based on a 'surface temperature, T_o or T_s, which actually corresponds to the temperature at the height of the (thermal) roughness length. If we assume an (ideal) stratified SL, stable or unstable, and perfect measurements with a given 'surface' latent heat flux, each measurement height would have a (slightly) different L_v (because of the temperature profile) and hence a different latent heat flux – which is inconsistent with the 'constant flux'. In order to obtain the 'surface flux' from a measurement at any height (within the SL, of course), we must therefore relate the conversion into energy units to a common height, i.e. the thermal roughness length. The actual task therefore in determining L_v is to find the temperature at the height of the thermal roughness length. I think the water temperature is a better estimate for this than the temperature at measurement height. This 'surface latent heat flux' then can be used to assess how many mm of water had been evaporated (what is one of the primary goals in the present study). In any case, two comments have to be added: first, it is in fact potential temperature that has to be used (more precisely, virtual potential temperature) – again the differences (for a 6 m level) are negligible. Second, if L_v is estimated using eq. (3), latent heat fluxes will be in [kW m-2] (since L_v is is in [kJ kg-1]) and not comparable to the sensible heat fluxes from eq (2).

Interactive comment

P6, l.17 . . .the mean vertical velocity: this could be mixed up with the mean vertical velocity over the averaging period that is zero in the 'double rotation approach'. In PF, it is the mean vertical velocity over the period that is used to define the plane.

P6, l.20 . . .calculations at sites..

P6, l. 28 below 0.5 'what'? (units)

P7, l.3 . . . data, which . . .

P7, l. 11 is reasonably good

P7, l. 28 is built

P9, l. 5 on longer time scales (or: on a longer time scale)

P8, l. 11ff The presentation of the methods to estimate evaporation is somewhat difficult to comprehend. I try to exemplify this for the Energy balance method. First, one is referred to Tab. 1. The first mentioned aspect of this method is, what is difficult to obtain. I then try to check (find) F_n in Tab 1 (which is apparently difficult to obtain). F_n does not appear in the given equation (but it is 'explained' below in the list of symbols – even if it is not present in any of the given equations). So, I cannot at least judge how this variable appears in the full equation or is related to other variables that do so. The same with the other neglected variables. The 'result' is called V0 (it has an 'X' in Tab 2). Estimating 'somehow' delta_Q produces then 'V2' (why is it V2 for this method, and V1 for the first?). Anyway, it also has an 'X' in Tab 2 (why not a V2?). And some of the other methods also have an 'X' in this table for delta_Q. Overall, I think the overview on the employed approaches should be presented in a much more concise manner. The reader should be able to judge what has actually been done.

P8, l. 15 to make the confusion complete, the 'third method' is then – not the third line in Tab 1 but the fourth. . ...

P10, l. 15 on 26% of the days

P10, l. 16 on about 57%...

P10, l. 18 if the along-valley flow is northerly (with what I concur judging from Fig. 1) the lake breeze would be expected to be perpendicular (easterly on the western shore). Wouldn't this mean that, what was called a 'lake breeze' before (p10, l. 11) is rather a superposition of the along-valley flow and the lake breeze? Can the authors comment on that?

P10, l. 22 in the beginning

P0, l. 25 on individual days

P12, l. 4 on most of the days ...

P14, l. 2 what does 'uncorrected' mean? No Webb correction, etc.? Or do the authors refer to 'only measured, no estimated (with the multiple regression model) values'? (same in Fig. 4). I think the term 'uncorrected' is not appropriate.

P15, l. 1 Maximum values are reached....: see major comment 2.

P15, l.11 the uncertainty...: so, how large was it found to be?

P16, l. 10 evaporation rates of 5.1 mm d-1, ... are measured: do these 'extreme days' contain any estimated values (using the statistical model)? How about the other 'large days' throughout the year?

P16, l. 14 not shown: I think that this 'case' (if it were shown) could serve as a good example to gain some confidence in the statistical model (see major comment 2).

Tab 6 'MD' must be defined.

P17, l. 16 MD is probably mean difference, right? Anyway, the mean differences are not given in the table to demonstrate this (for V0 they are essentially the same...).

P17, l. 20 I suggest to start a new paragraph for the BREB method.

P17, l. 25 the largest

P25, l. 30 22%: judging from Fig. 7 this number is probably valid for a 28 d averaging period

p25, l. 31 coefficients

p25, l. 31 improved the results: I do not really agree. Indeed, the correlation coefficients do somewhat increase (but look at Fig. 6 – both versions would probably serve as examples for 'statistics 101' students for data sets, for which a linear model is not appropriate). At the same time, slope and offset are getting worse (this is why we usually use different statistical measures....). In my view the results of V2 (as compared to V0) simply demonstrate that the calculation of heat storage is not appropriate (see major comment 1)

p17, l. 34 11%: same as above (and also in the following) these values seem to apply for the 28 day averages

p19, l. 7 up to 100%: I don't think I can see this from Fig. 7d

p20, l. 16 with a heat storage term....: in fact, I only now understand what actually V6 does: it fits the Penman equation to account for the missing storage term, right? So, how is this fitting process being done? If it is fitted – as I assume – using the measured evaporation, it does not come as a surprise that a negligible mean difference results. The results from this exercise have to discussed in this light (major comment 3).

p21, l. 8 due to the much higher

p22, l. 12 The BREB, Priestly-Taylor and Penman... All these methods do not employ radiation (which has a different field of view – and does not represent the water surface, see major comment 1). I would hypothesize that this is, in the first place, the reason for their bad performance. Only if the Penman method is fitted to the data, it can also produce some reasonable results.

Reference Arya SP (1988)ÂăIntroduction toÂăMicrometeorology, Academic Press (San Diego), 1988. No. of Pages: 307

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