

Interactive comment on “Observations of positive sea surface temperature trends in the steadily shrinking Dead Sea” by Pavel Kishcha et al.

Anonymous Referee #2

Received and published: 22 May 2018

In this paper Kishcha et al. use satellite-based Sea Surface Temperature (SST) from a 17-years long record (2000–2016) using MODIS, with the aim of interpreting the long-term changes (trends). The study area is the Dead Sea and surrounding land.

According to the paper, the water level of the Dead Sea dropped significantly in the last 40 years, as a consequence of three factors: 1) decreasing water inflow from the Jordan River; 2) decreasing tendency in rainfall; 3) increasing evaporation.

The authors in this study focuses on the sea surface temperature parameter. Using satellite-based SST, they observe a statistically significant positive trend in Dead Sea at both day time and night time. Previous studies suggest that this increasing SST would be due to increasing surface solar radiation as a result of decreasing cloud cover.

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The authors here analyse surface solar radiation measured together with near-surface wind speed from a hydro-meteorological buoy deployed in the Dead Sea. In addition, the author use yearly data of Dead Sea water levels based on available measurements from 1992 until now.

From those pointwise measurements, they find the absence of positive trends in surface solar radiation. This means that the observed positive trend in the daytime SST cannot be explained in terms of surface solar radiation trends. They also do not observe statistically significant trend in near surface wind. They interpret in stable mixing over time (as more mixing would produce more evaporation). Measurements of water level clearly indicate a negative trend over the observational period.

In my opinion, the reasoning of the authors to draw conclusions is uncomplete, i.e., that the evidence that “the observed increase in the Dead Sea SST over the study period cannot be related to increasing surface solar radiation”, supports that this SST trend is the net result of two opposite processes: 1) increased evaporation that results in decreased SST in the long-term; 2) reducing water surface that leads to some additional surface heating (increased SST) every year.

However, a number of factors need to be considered:

1) warming of waters produces expansion and the water level should increase. However, this effect is much more evident in big oceans than in shallow inland water bodies that are subjected to marked year to year changes. A warming atmosphere produces more evaporation, meaning more water is available for precipitation.

2) If the level drops, the density and saltiness might rising until a certain point where the rate of evaporation will reach a kind of equilibrium.

3) the evaporation of water depends on open water surface. For instance the water surface area influences the quantity lost through evaporation from water (if the water surface decreases less surface is exposed to atmosphere). The authors look at one

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single point that is not enough to draw conclusions about wind effects. This point needs to be better supported (e.g. measuring the water surface area change over time)

4) Evaporation is influenced by the temperature of air above (higher air temperatures favour the rate of evaporation). But also the vapor pressure of the liquid has to be considered. The rate of evaporation therefore depends on the difference between saturation vapor pressure at the water temperature and at the dew point of the air. Higher the difference, more the evaporation.

5) Another comment is about the wind. More than amplitude, evaporation is influenced by surface roughness. Moreover, nothing is said about air pressure. I would expect decreasing evaporation with increasing pressure.

6) Another point is that most of the water volume of the Jordan River is extracted before the river reaches the Dead Sea. How much is contributing to the lake drop? It is important to know before saying evaporation is main contribution to the decrease of the water level.

To conclude, the topic is certainly interesting, but authors are missing parts that could influence their conclusions. In my opinion, they have to better describe the occurring meteorological, hydrological, and geophysical processes and their interactions with the related variables that could play a key role. Also, the bibliography is just mentioned and somewhat missing (see below where there is important discussion about solar radiation and wind effects) and should be better explained how some conclusions are stated about water level drops, evaporation, winds, etc.

e.g. Lensky, N. G., Lensky, I. M., Peretz, A., Gertman, I., Tanny, J., & Assouline, S. (2018). Diurnal course of evaporation from the Dead Sea in summer: A distinct double peak induced by solar radiation and night sea breeze. *Water Resources Research*, 54(1), 150-160.

Overall, the paper calls for some major revision. I like reading the revised version.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2018-31>, 2018.

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