The paper “Near East Desertification: impact of Dead Sea drying on convective rainfall” by Samiro Khodayar and Johannes Hoerner presents an interesting analysis of the possible impact of the absence of the Dead Sea on the precipitation regime in the region. It is done using two simulations of 11 years (2003-2013) with the high spatial resolution (2.8 km) COSMO-CLM model, where one simulation is the reference and in the other the lake area is replaced by a soil surface. Two case studies are also considered in more details.

I have some major concerns that need to be addressed as detailed below:

Major comments:

1) Modeled mean annual precipitation: The mean annual precipitation computed by the model (Figure 5b) is quite different from observations, both in absolute magnitude and in gradients. At this region, the mean annual rain near the Mediterranean shoreline is in the range of 400-600 mm/year while over the higher topography west of the lake it can reach 500-700 mm. In the simulations presented in Figure 5b the range is from <75 mm/year at a close distance to the Mediterranean shore to 125-300 mm/year at the high topography west to the Dead Sea. The model presents much drier conditions and much larger gradients and seems not to represent well the typical more intense rain near the shore. The general effect of distance from sea on precipitation is not captured, while the orographic effect is probably well simulated. Although this paper is not focused on the effect of the Mediterranean Sea, still, as the main source of moisture to precipitation in the region, including in the study area, it is of concern that total amounts and gradients of precipitations are not represented well. The authors do not refer to this important deviation at all, not to mention explain why is it so high and why is it not harming the validity of the results and conclusions.

2) Dead Sea representation in the model: the lake form shown in Figure 1b is very noisy and different from the real lake shape and coverage. I understand this is how the lake is seen in the global data set of land use but the authors still could manually apply the actual lake shape. Furthermore, it is not stated anywhere in the paper if the salinity of the water was account for. The very high salinity reduces substantially evaporation rate compared to fresh water. Another important aspect is water temperature. What was used? This also can affect substantially evaporation and it is very different from the Mediterranean Sea temperature. All these features – lake shape, water salinity and temperature must be addressed as this is the most important feature in the simulation. The authors should note there are publications on the Dead Sea evaporation rate (e.g., Hamdani et al., 2018), so the simulated lake evaporation in the REF run can be verified.

3) Dead Sea abundance simulation: for the sensitivity analysis simulation the authors replace the lake with a soil at an elevation of 405 m below mean sea level, stating that this is the depth of the Dead Sea in the external data set, GLOBE. I find this quite strange as presently the lake level is at ~430 m below sea level; the lake’s bathymetry is characterized by steep slopes and wide, flat lake floor at 720 m below mean sea level (see for example Sirota et al., 2017 among many other publications about the lake). So it is not clear what does the height of 405 m represent; if the Dead Sea will dry out, most probably the surface will be at a much lower height. Furthermore, the high gradient slopes exposed as a result of this drying can possibly affect precipitation, which is presently not considered in the paper. Also, please note, some studies

claim it will not dry out but will get to a new (possibly much lower) steady state level (Yechieli et al., 1998).

4) Dead Sea moisture transport and winds: it could be very helpful to give some background on the prevailing winds in the region and, if possible, on tracks of Dead Sea-originated moisture, possibly by backward moisture tracking analyses. For example, as the western component is mostly positive in wind direction, changes in precipitation patterns associated with Dead Sea absence are expected to be much stronger east to the lake than at its west side. This aspect is mentioned for the two case study analyzed but not in the climatologically sense.

5) Separating real effects from noise: it is hard to tell what of the effects presented in the paper are real and what are part of a noise or random error. Although the two model runs receive the exact same lateral and initial conditions, still, some differences could result from small numerical effects, not related to the Dead Sea absence. Especially, if one considers the argument in 4, above, it is not expected to have symmetrical differences on the west-east axis; however, Figure 5b (right) looks very noisy and the noise seems to have a similar pattern west and east to the lake. Could it be this noisy field of precipitation differences between the two simulations is random errors? one way to check this is to build the distribution of random differences by repeating the reference simulation few times and then consider only differences between the SEN and the REF simulations that are out of the 0.95 quantile.

Specific comments:

6) In some of the figures (e.g., Figure 2) evaporation is computed over land and lake areas and such results are hard to interpret. Obviously, the lake pixels have very high evaporation in the REF simulation and very low evaporation in SEN simulation. Could it be that this is the main control of the total volume difference between the two simulations? or, alternatively, it is just a small fraction of the total volume difference? if computation is done on land pixels only, it would be more informative in my opinion.

7) Please consider presenting the differences also in a normalized form. Are the changes described negligible or substantial? presently, it is hard to tell.

8) The authors describe in the introduction the lake level decline, which is presently > 1 m/year, but they do not state clearly that this decline is due to the massive water consumption at its upstream. One may get the impression that this substantial lake level decrease is due to climate change; this is wrong. It is possible of course that climate changes have a contribution to the lake level decrease during the last decades but it can explain much smaller decline rates comparing to the effect of water use (Lensky and Dente 2015).

9) The model spatial resolution is high, 2.8 km, and at this resolution convection can be resolved directly. However, not sure this is also true for shallow convective. Can you provide some info how was shallow convection handled? Another question is whether 2.8 km is small enough for small-scale convective typical to the Dead Sea manifested for example in the small convective rain cell size (e.g., Belachsen et al., 2017).

10) L101: Note that high resolution modelling in the region was performed by few studies, including: Hochman et al. (2018), Rostkier-Edelstein et al. (2014), Kunin et al. (2019) and possibly others.

11) L290: "…almost no difference…". I may have misunderstood the sentence, but it seems to me there are large differences in simulated evaporation in REF and SEN for A1 and A2 (Figure 3b). Also, it seems as there is more evaporation in the absence of the Dead Sea. Could it be because of the higher 2mT? Maybe there is also a change in the wind regime that could contribute to this?

12) L351: can you explain the differences in 500 hPa geopotential height?

13) L358-359: how many instances does a probability of 1^e-6 represents? Could it be a single occurrence, possibly by chance?

14) L439: how MSB is differentiated from the cyclone-related wind? Does ground temperatures in this day hotter or colder than SST? Could the decreased wind near the Dead Sea be related to the higher friction caused by the change in land use? Wouldn't this differ if the ground was set to 700 m below mean sea level rather than 405 m?

15) L456: This is a good point. However, what is the temperature of the Dead Sea surface in the REF simulation? Isn't the opposite effect expected, since the Dead Sea surface temperature in November is ~25 oC (e.g., Hamdani et al., 2018)?

Minor comments

16) L183: The statement about L (from SAL) is not accurate. It measures the distance of the center of mass of precipitation from the modelled one, and the average distance of each object from the center of mass.

17) L286: north-west instead of north-east for A1

18) L307: mm per day?

19) L330: a better citation for lake evaporation would be Hamdani et al., 2018

20) L332: evaporation is probably correlated with rainfall which in turn correlated with topography. Also, soil type is often correlated with topography and rainfall.

21) L368: correct zero 0.

22) L406: gradient units should not be per km?

23) L457-458: it is hard to see the "near-surface" temperature in Figure 11, since it is plotted from 1000 hPa, while the Dead Sea is at ~1060 hPa.

24) Figure 7 caption: please check. left and right of 7a are not the REF and REF-SEN.

25) Some of the figure units should be corrected. For example, mm to mm d^-1.

References:

Belachsen, I., Marra, F., Peleg, N., Morin, E. 2017. Convective rainfall in a dry climate: relations with synoptic systems and flash-flood generation in the Dead Sea region. Hydrology and Earth System Sciences, 21, 5165-5180. doi:10.5194/hess-21-5165-2017.

Hamdani, I., Assouline, S., Tanny, J., Lensky, I.M., Gertman, I., Mor, Z. and Lensky, N.G., 2018. Seasonal and diurnal evaporation from a deep hypersaline lake: The Dead Sea as a case study. Journal of hydrology, 562, pp.155-167.

Hochman, A., Mercogliano, P., Alpert, P., Saaroni, H. and Bucchignani, E., 2018. High-resolution projection of climate change and extremity over Israel using COSMO-CLM. International Journal of Climatology, 38(14), pp.5095-5106.

Kunin, P., Alpert, P. and Rostkier-Edelstein, D., 2019. Investigation of sea-breeze/foehn in the Dead Sea valley employing high resolution WRF and observations. Atmospheric Research.

Lensky, N. and Dente, E., 2015. The hydrological proecesses driving the accelerated Dead Sea level decline in the past decades. Geological Survey of Israel Report.

Rostkier-Edelstein, D., Liu, Y., Wu, W., Kunin, P., Givati, A. and Ge, M., 2014. Towards a high-resolution climatography of seasonal precipitation over Israel. International Journal of Climatology, 34(6), pp.1964-1979.

Sirota, I., Enzel, Y. and Lensky, N.G., 2017. Temperature seasonality control on modern halite layers in the Dead Sea: In situ observations. Bulletin, 129(9-10), pp.1181-1194.

Yechieli, Y., Gavrieli, I., Berkowitz, B. and Ronen, D., 1998. Will the Dead Sea die?. Geology, 26(8), pp.755-758.