Answers to Anonymous Reviewer #2

"Near East Desertification: Impact of Dead Sea drying on convective rainfall" by Khodayar and Hoerner submitted to Atmospheric Chemistry and Physics

Dear Reviewer2:

We have corrected this manuscript following all your comments and suggestions. In the following you can find a detail answer to all your general and specific comments.

Kind regards

Samiro Khodayar

General Comments:

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.

We have performed an extensive analysis/validation of the precipitation field using EOBS and APHRODITE information, the results show in agreement with other modelling efforts in the region a general underestimation with particular focus on the near-coast flat areas and better results over the complex areas. We have demonstrated that neither the simulations domain, the forcing data or the grid spacing used are the main reason for this bias. Indeed, closer results are obtained for the finer resolution simulations. Revising past simulation and validation exercises in the region in past publications we found out that similar biases have been identified in the past, also with different model schemes. In these publications it is pointed out inaccuracies in the SST as the reason for the biases in the precipitation field in the Mediterranean coastal area. Generally, relevant inaccuracies are identified in the SST field forcing our simulations, which have been demonstrated in the past to have a significant impact in the simulation of precipitation in the region. It is out of the scope of this paper to demonstrate this relevance, but we do agree with the reviewer that this is a relevant point to be discussed and to be investigated. Therefore, we *have included this discussion in the manuscript and proposed in the conclusions the need to investigate, for example though sensitivity experiments the relevance of SST to obtain a more accurate precipitation field.*

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.

We agree with the reviewer that differences exist between the reality and the modelled Dead Sea, whose characteristics are given by the global data set. It was out of the scope of this article to improve this representation in our model simulations, but we agree with the reviewer that this is a relevant point. Therefore, we will investigate this point and the sensitivity of our simulations to this in the new simulations we are performing in the area as indicated in the conclusions. The salinity is not considered in the Dead Sea simulations. Nevertheless, in the PhD thesis of Jutta Metzger/Vüllers, and the corresponding paper,

Wind Systems and Energy Balance in the Dead Sea Valley, 2017 | dissertation-thesis, DOI: 10.5445/KSP/1000072084

Dead Sea evaporation by eddy covariance measurements vs. aerodynamic, energy budget, Priestley–Taylor, and Penman estimates. Hydrology and Earth System Sciences. 2018-02-09 | journal-article, DOI: 10.5194/hess-22-1135-2018

it has been demonstrated that wind speed and vapour pressure deficit (humidity) are governing factors for evaporation in the Dead Sea valley, being the influence of salinity low as assessed by measurements and calculations. During the measurement campaign of the DESERVE project evaporation measurements were performed for a period of one year, which have been used in this publication as reference. We have included in the conclusions some sentences pointing out these issues raised by the reviewer, which we agree have to be considered but were out of the scope of this study.

3) Dead Sea abundance simulation: for the sensitivity analysis simulations 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).

The remaining flat floor of the lake at some level above 720 m will be much smaller than the actual lake area. The dry level will be higher than 720 m because of the huge amount of NaCl in the valley. We agree with the reviewer that there is discussion about the possibility of the lake never drying out, they indeed point out that a wet swamp of semi-crystalline salt would remain, even if there is no more inflow of fresh water in the valley. It was out of the scope of this paper to explore or discuss this or further possibilities. However, we agree with the reviewer about the relevance of this point, for that reason in the new set of simulations *we are performing (follow-up article) we are considering "intermediate condition/situation of the Dead sea" in addition to the more extreme condition, totally dry, investigated in this publication.*

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.

The article from Metzger et al. (2017) investigates in detail the climatology of the winds in the region. This information and corresponding reference is included in the article. It is not possible to recalculate backward moisture trajectories over the simulations performed given the resolution of our output and the impossibility of reproducing the simulations. External Lagrangian schemes could be used such as the freely available HYSPLIT software; however, this uses different model information that the one in this publication and validation will be needed to demonstrate the consistency of the results. We intend to include these calculations in the follow-up simulations we are performing in the region to complement this information.

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.

Unfortunately, it is not possible to repeat the reference simulation since the computation system in which we run this simulations is not available anymore. However, we can confirm that at the moment of realization of these simulations we did run the same simulation in two different machines and we obtained the same results. Also the fact that we observe the same results in the 10-year long simulations and in the events simulated for several days, furthermore in many different events confirms that the effects presented in this paper are not random errors or noise.

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.

Evaporation is only computed over land in Figure 3 to facilitate the interpretation of the results.

Figure 2: all GP for all calculations are considered. In the following the example of evaporation including all GP(left) and only land points (right) is shown.

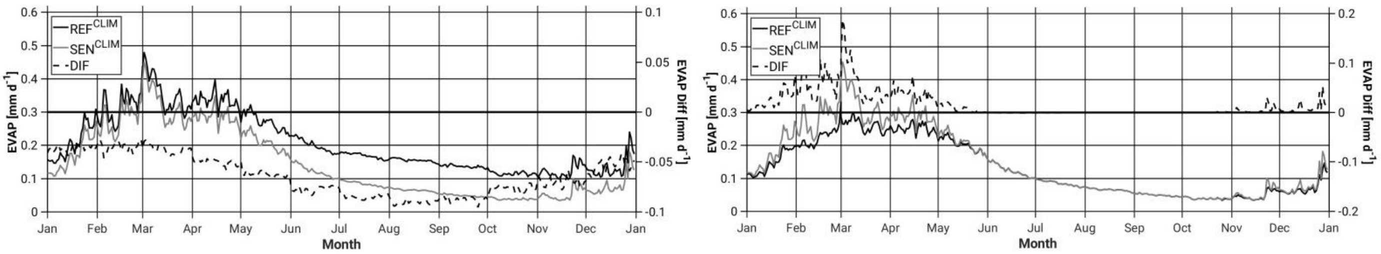
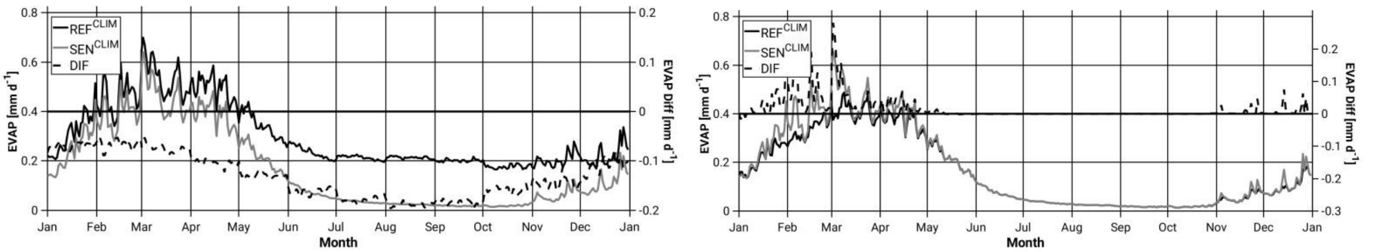


Figure 3: Only land points for evap and all GP for the rest

The Dead Sea grids have very high evaporation in the REF simulation and very low evaporation in SEN simulation, this difficult the interpretation of the results, thus we removed this effect in Figure 3 for evaporation, also because of the separation in 4 subdomains. In the following the example of evaporation including all GP(left) and only land points (right) for Area 1 is shown.



7) 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).

This information has been included in the manuscript as we agree with the reviewer this is relevant for the readers.

8) 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).

This information has been included in the description of the model.

9) *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.*

This information has been included in the text.

10) 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?

Evaporation difference is negligible in the May to November period.

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

Differences in the near-surface conditions impact surface pressure as well as wind circulations, this information is transported upwards in the atmosphere locally and remotely, which results in the weak changes in the upper-atmospheric levels in this case the 500 hPa geopotential height.

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

A probability of 1^e-6 represents approximately 22 instances. The total number of instances is 21759840, so one instance would be represented with a probability of approximately 5^e-8.

13) 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?

The timing, characteristics and evolution of the horizontal and vertical air flow helped us identify the MSB. Ground temperature in this day varies between 18° and 31°, whereas temperature over the Dead Sea varies only slightly between 25-26°.The temperature difference between the cooler maritime air mass and the warmer valley in REF result in the downward penetration of the MSB.

To give an accurate answer to the last two questions we believe it would be necessary to perform some sensitivity experiments to demonstrate these hypothesis. However, we believe that the change in land use is a contributing factor to the decrease wind, but not the only one, and the depth of the Dead Sea in the SEN simulation would not have changed the observed dynamics. It would have rather enhanced the behaviour to more marked temperature differences.

14) 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)?

The ground temperature in SEN is higher than the surface temperature in REF between 8UTC and 13UTC. At point B, the mean surface temperature on the 18.11.2011 in REF is about 26°, whereas the mean ground temperature in SEN is about 19°.

Minor comments

15) 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.

Corrected

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

Corrected

17) L307: mm per day?

mm per month, as indicated in the caption of figure 4 monthly mean values calculated using daily mean values are presented. This has been corrected.

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

This reference has been included

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

Yes, we agree.

20) L368: correct zero 0.

Corrected

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

Corrected

22) 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.

Unfortunately, this is the lowest level available as pressure level.

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

Corrected

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

This has been corrected.

References:

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