Answers to Anonymous Reviewer #1

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

Dear Reviewer1:
Thanks for your comments and suggestions. We have considered all of them and improved the manuscript accordingly. In the following you can find a detail answer to all your general and specific comments.
Kind regards
Samiro Khodayar

General Comments:

1. I do not think that you can say that you investigate the impact of drying on convective rainfall. You use only daily model output, while convective events are short-term and local events, and can thus be lost in daily output. I would thus recommend to analyse the hourly output for the entire 10-year period, and not only for two events. However, as I understand from the manuscript you have saved daily output only for that simulation so analysis of hourly events across decade-long simulations would not be possible without rerunning the simulation. I wonder if you should then at least change the title into something like "Near East Desertification: Impact of Dead Sea drying on the water cycle" or "Near East Desertification: Impact of Dead Sea drying on the water budget" or "Near East Desertification: Impact of Dead Sea drying on the local climate".

We agree with the reviewer that the ideal approach would have been to analyse the hourly output for the entire 10-year period. Unfortunately, as specified in the manuscript we initially only saved daily output because of the storage capacity since our initial purpose was to assess the impact of a drying Dead sea on the climatology of the region. However, after careful inspection of our results we found interesting impacts on the precipitation field, particularly on severe events mainly of convective nature (which are rare but relevant in the area) and even more interesting results when analysing the underlying mechanisms. Even though only daily precipitation was available for the entire 10-year long simulation, the convective nature of the investigated cases was clear due to the isolated situation of the events investigated as well as their characteristics such as high local convective available potential energy. Following the need for higher temporal information new simulations with hourly outputs were performed. The approach as well as some of the results obtained is novel, therefore, we believed in the relevance of publishing this study.
In a follow-up publication covering a 20-year period, hourly outputs are being saved for the entire simulation. This will allow us to come back to the points raised by the reviewer. Indeed, in this follow-up publication we will investigate in more detail the impacts on the local climate. Therefore, regarding a change in the title and following the reviewer suggestion we propose the following
Near East Desertification: impact of Dead Sea drying on the local conditions leading to convection.

A comment has been included in the manuscript to clarify that the daily output supposes a limitation.

2. Why do you use high-resolution convection-permitting simulation since you analyse only daily output on decadal time-scales and daily statistics is already well represented with the coarser resolution model? The studies that you cite show that the largest benefit of using such a high resolution is at the sub-daily time scale.

Even when using daily outputs the use of high-resolution convection-permitting simulations is beneficial for a better representation of model characteristics and atmospheric processes leading to convective precipitation, such as topography, secondary wind circulations etc

Although the main benefit of high-resolution convection-permitting simulations versus parameterized convection simulations is at sub-daily time scales, particularly for summer period, as adequately pointed out by the reviewer, daily precipitation has also been seen to be affected and improved, particularly in winter time (Fosser et al. 2014).

Fosser, G. & Khodayar, S. & Berg, P.. (2014). Benefit of convection permitting climate model simulations in the representation of convective precipitation. Climate Dynamics. 44. 45-60. 10.1007/s00382-014-2242-1.

Moreover, high-resolution convection-permitting simulations on shorter time scales with hourly output are used for further investigation of underlying mechanisms leading to heavy precipitation in the area of investigation. This allows the consistency between the simulation of the events at both simulation schemes.

We agree with the reviewer that this is a relevant point, so we included a comment with respect to this point in the manuscript.

3. The manuscript would further benefit from a better explanation of the domain that you simulate. I do not see how many grid points you have in x and y direction, and how large is the relaxation zone which you should take out from the analysis. How many grid points do you have at the end for the analysis? My rough estimate leads to a smaller number, so the influence of the domain size has to be discussed.

In Figure 1a, the simulation domains at 7 km and at 2.8km km as well as the investigation domain or study area are shown, to complement this information further details such as the number of grid points in x and y direction have been included in the manuscript as suggested by the reviewer.

The 7 km run covers a box of 250 x 250 grid points, the 2.8 km run covers a 150 x 150 grid points box, 22500 in total, and the study area, 72 x 92 grid points, leaving between 20-40 grid points as relaxation zone in the north-south-east-west direction.

The influence of the domain size on the simulation and analysis has been already pointed out in literature for different regions. In this study we have discussed with experts in the region and considered past studies in the area to make sure that our larger domain is well located and large enough to have into consideration all possible relevant synoptic situations as well as the Mediterranean sea impact relevant for the development of extreme phenomena in the study area. This explanation has been included in the manuscript.

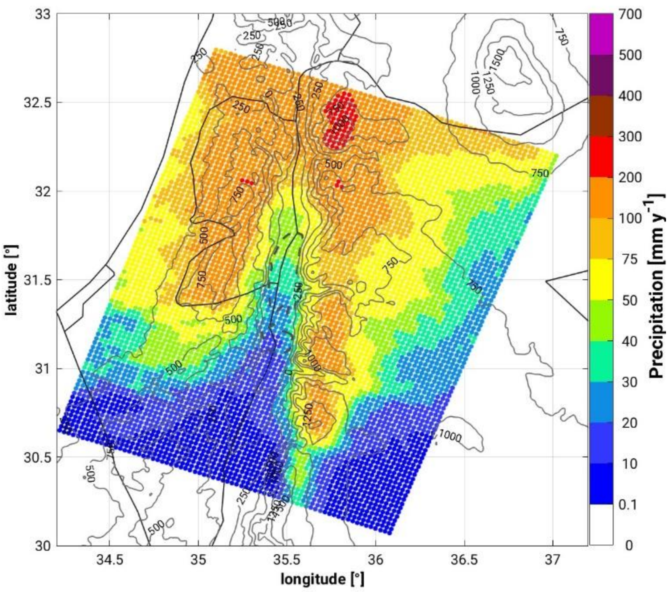
4. Since you are doing the sensitivity experiment, I wonder if you really need a 10- year long period and if you could already address the problem with 1-5 years long simulations. With reducing the number of simulated years, you could use a larger domain.

In our sensitivity experiment we have seen that at least 1 to 2 years spin-up time are needed. After this consideration we agree that shorter time periods could be beneficial when particular situations/periods/events have to be investigated, particularly regarding the computational time and costs of the study. After discussion with experts in the modelization of the area regarding the size of the larger domain no benefit has been found in doing so. However, the time period considered is highly beneficial for the climatic aspects considered in this analysis, which provides a novel perspective of the conditions in the area.

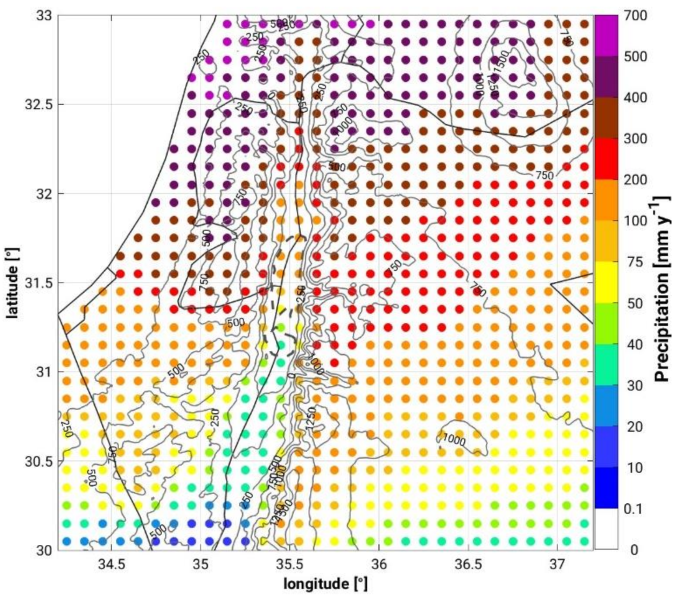
5. How is the model performing over that region? You do not show any validation of the results for the reference simulation, so why should we trust the model? You even state on page 4, line 101-102 that this is the first attempt i.e., a convection-permitting model is for the first time applied in that region, so the manuscript should for sure present some evaluation results. You already mention some papers with the observations, so maybe these can be used. Or in the absence of high-resolution observations, EOBS observations can be used as well. Of course, one should be aware of and take into account the uncertainties for different regions and fields.

In general, the observations in the area are scarce in time and space. We performed some initial comparisons with the CMORPH satellite precipitation product; however, no comparisons were included in the manuscript due to some strange values over the Dead Sea region. No validation of this satellite product has been attempted in the region to the authors knowledge.
Following the reviewer suggestion, we performed comparisons with EOBS data set despite the coarse resolution of the later, 0.1º, and the indication by experts in the region of the bad performance of this product in the area.

*2.8km CTRL model_*
mean over the 2004-2013 period



EOBS_data set
Same period

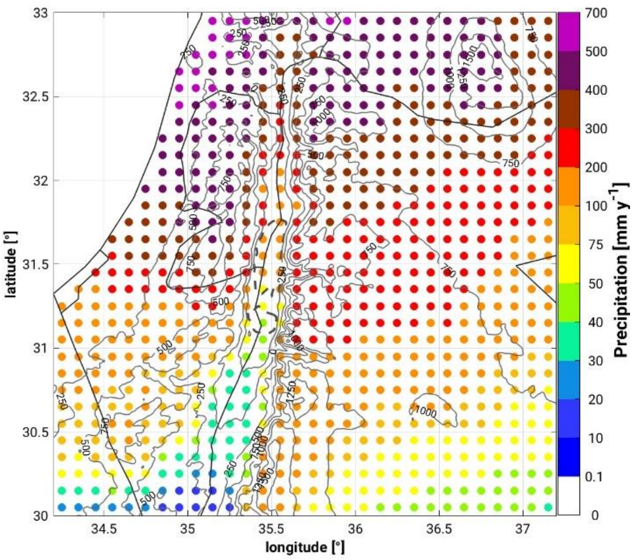


This comparison points out a general underestimation of precipitation in the north and particularly near the Mediterranean shoreline, but correctly captures the north-south gradient in the area.
This suggests that the model well simulates the orographic effect, while the general effect of distance from sea on precipitation is not well captured. Both the 7 km and the 2.8 km runs exhibit the same performance, thus, discarding a relationship of the biases with the grid spacing. Nevertheless, one may notice an improvement in the finer model resolution, particularly over topographic areas.

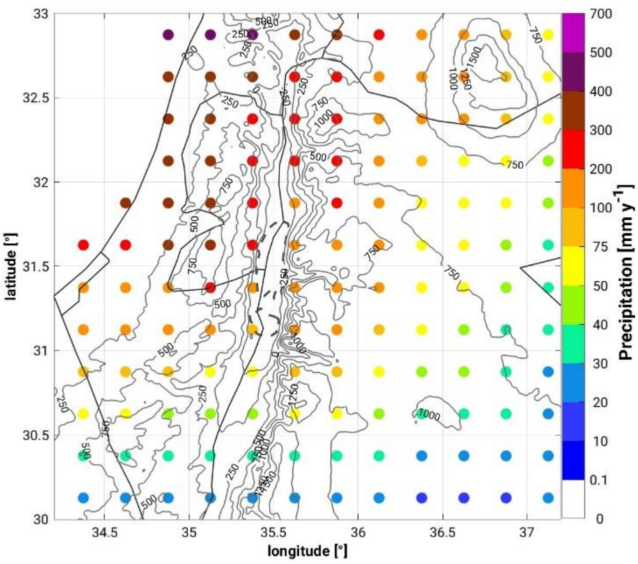
An additional comparison has been performed with the APHRODITE's (Asian Precipitation - Highly-Resolved Observational Data Integration Towards Evaluation) daily gridded precipitation which is the only long-term continental-scale daily product that contains a dense network of daily rain-gauge data for Asia. It has a resolution of 0.25º and is available for 1980-2007. The advantage is that it includes more rain gauge stations and it is a product widely used for validation purposes in this region of the globe. We compared the data from 2004-2007 with the respective data from the 2.8km simulation and EOBS. Please be aware of the different colormap scale between the EOBS/APHRODITE and the model simulation precipitation fields.

The Aphrodite data shows lower precipitation values than EOBS, but still higher than our simulation particularly close to the northern Mediterranean shoreline, over coastal-flat terrain, whereas the best agreement is again at areas dominated by complex terrain. This agrees with previous high-resolution modelling activities in the region with different models such as Rostkier-Edelstein et al. (2014) using WRF at 2 km. They suggest in this publication that inaccuracies in the gridded SST dataset used in the simulations could be responsible for the observed bias pointing out the strong sensitivity of precipitation in the Mediterranean basin to very small differences in the SST (Miglietta et al. 2011). Contrary to these results, Hochmann et al. (2018) showed with the COSMO-CLM model at 8 km resolution and driven by CMCC-CM against APHRODITE, a west-east pattern of overestimations in the coastal plains and underestimations in the mountainous regions in the seasonal precipitation, especially in the winter months (DJF).

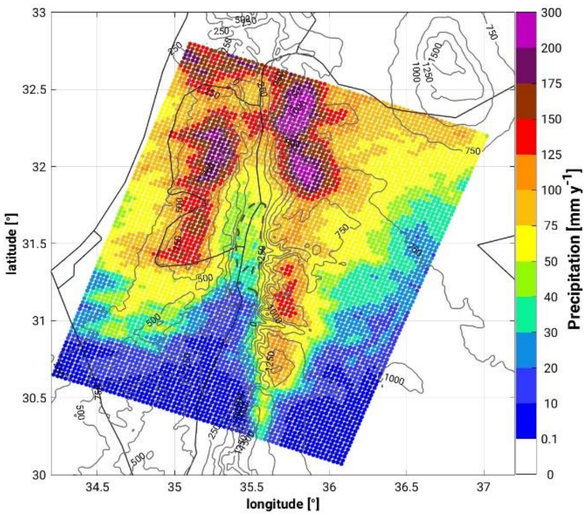
EOBS



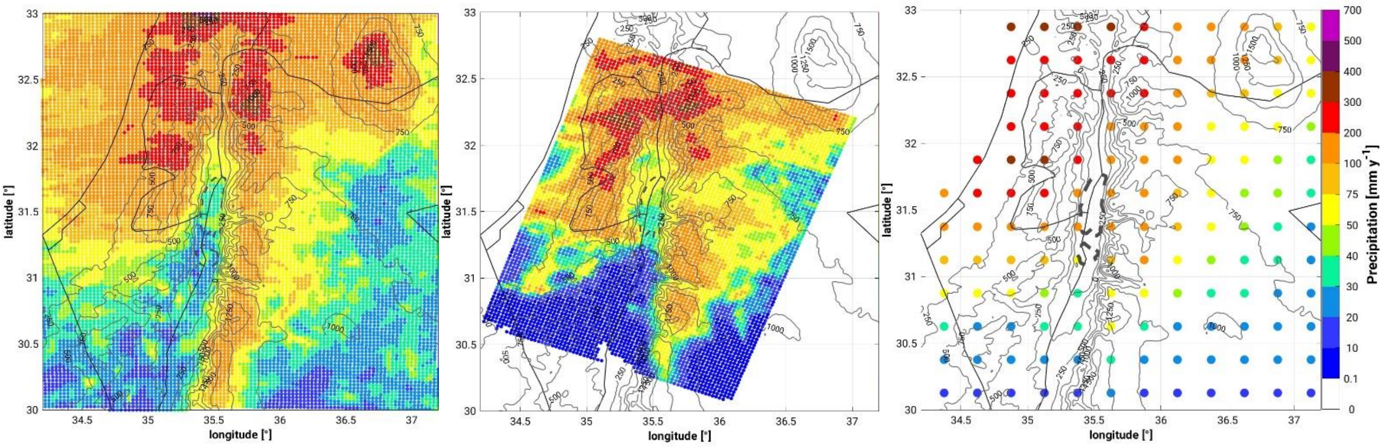
APHRODITE



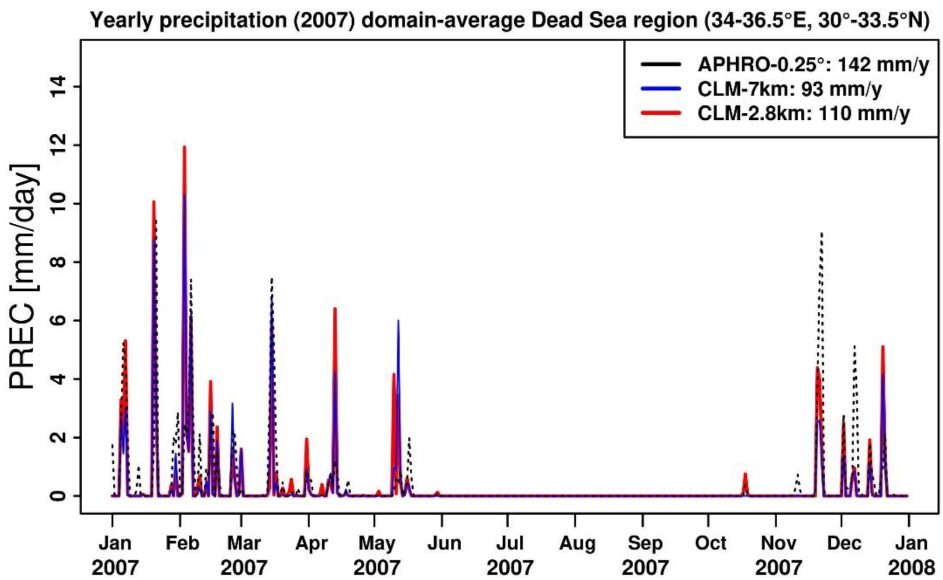
2.8km-CTRLsimulation-IFS forcing



We performed an additional comparison for the year 2007, between the 2.8km-CTRL simulation-IFS forcing from the present study and the new simulations we are performing for the region, with hourly outputs covering the 2006-2018 period and forced by the newly available hourly ERA-5 data, also with a larger *simulation domain for the 2.8 km simulation. Our intention was to assess the possible impact of using different forcing data and/or domain of simulation. Our results show quite similar results between both simulations and similar biases with respect to the APHRODITA dataset, particularly at the Mediterranean shoreline, although a small improvement is shown in this area in the new model realization.*



The comparison of the temporal evolution and the yearly sum for the year 2007 shows that in general the COSMO-CLM model both at 7 km and 2.8 km, quite well represents the precipitation events in the region.



We will discuss this in the paper and additionally indicate in the conclusions that further improvement/refinement in the model simulations in the region is needed despite the improvement seen by using higher resolution convection permitting model simulations.

6. Throughout the manuscript, you use the difference between the REF and SEN experiments, and you calculate it as REF-SEN, which is a bit strange since it is common to use the reference simulation, in your case REF, as a subtrahend. This would make a discussion and figures easier to follow.

This has been changed throughout all the manuscript. Changes include Figure 2, 3, 4 5 and 7.

7. I do not think that the heavy precipitation events that you analyze are well chosen. You take two events that have the same synoptic patterns, while in the introduction you mention that heavy precipitation events are associated with the three main synoptic patterns. The two chosen events are only a few days apart and their connection is not discussed. It would be more interesting to choose 1-2 events for each type and then analyze them. These would lead to more meaningful results.

We agree with the reviewer that the selection of events could attend different motivations, for example different events associated to different synoptic patterns. However, in this case it was not our motivation or purpose to investigate convective situations under different synoptic patterns, but rather cases in which the mechanisms leading to the observed differences in the precipitation field between the reference and the sensitivity experiment are noticeable different at the local-to-mesoscale. As indicated in section 3.1 and Figure 6, the selected situations where those showing a larger deviation regarding SAL (Structure-Amplitude-Location) components between both simulations. Nevertheless, we generally investigated all cases listed in table 1 to have a general idea of the mechanisms leading to the differences; however, it was out of the scope of this paper to analyse all in detail.
A relevant point indeed is the possible connection between both events, thanks for pointing out this. We did not discuss this in the text, but we did investigate this point during the analysis of the cases finding out that in the period in between both events no atmospheric differences could be found between the simulations. This information has been included in the text.

8. Some plots are really difficult to read in the printed version, especially in Figure 3. In addition, not all that is shown on the plots is explained in the captions (for example in Figure 4). Please do a better caption and work on the visibility of the plots.

We have improved the quality and size of most figures to improve their visibility. Additionally, we have carefully examined all captions to include relevant information that was missing.

Specific comments:

1. Page 2, line 5-6: “Perturbation simulations. . .” I would call them “Sensitivity simulations. . .”

Changed

2. Page 2, line 13-14: You only look into the sensitivity on the presence of the lake, not really the future warmer climate. For that, you would need to modify your experiment.

I would thus suggest here to explain only the influence of the lake presence, and in the final line, you can explain what would that mean for the future warmer climate.
We agree with the reviewer that we are not simulating the future, however, literature agrees that the future climate in the region includes a drier Dead Sea. In this sense, the expected ongoing lake level is expected to have the described consequences on the local climate. To clarify this point and avoid any misunderstanding we rewrote the paragraph following the reviewer’s suggestion.

3. Page 2, line 15-16: I do not think that you show that.

This is not explicitly shown in the manuscript, mainly because the differences are not significant. Therefore, following the reviewer's suggestion regarding this point we removed this information from the abstract.

4. Page 2, line 21-23: Why on many occasions, if you find/show that for only one event?

Even though we just show as example a more detailed analysis of two selected cases, we did investigate for all other events listed in Table 1, which were the main mechanisms leading to the differences between both simulations. This was also to evidence that the cases examined were not unique. This information has been included in the manuscript.

5. Page 3, line 39-40: A bit strange line. Please rewrite. Also, if the influence of Dead sea on local climate is already known, why do we need another study on it.

The paragraph has been rewritten.
Even though the influence of the Dead Sea on local climate has been evidenced in several publications starting in 1939, Ashbel et al., the advances in the last decade regarding observational and computational capacities allow us to better understand the consequences of the sea level decline, which is furthermore a continuous process rather than a static change.

6. Page 3, line 61-65: I have the feeling that these two lines are describing the same but still say different. Please synchronize it, or if the different studies say different things, please mention it to be clearer.

Modified

7. Page 4, line 78: by ". . .these events. . ." you mean ". . .these heavy events. . ."

Included

8. Page 4, line 88: As already mentioned above, you look into the sensitivity of climate the presence of the lake and not the climate change.

Modified

9. Page 5, line 120-122: This part needs a better explanation of the model setup. The 7km and 2.8 km domains are different (as shown in Figure 1). How many grid points do you use for each of them? How does 7 km and 2.8 km model differ in model physics? Do you use the parameterization of convection in 7 km or not?

A paragraph is included extending the information regarding the 7 and 2.8 km runs.

10. Page 5, line 122-125: Here you say that you are using ECMWF IFS as a driving data for 7 km model, and later on page 6 (line 148-150) you say that the reanalysis is used. Please clarify.

This has been corrected.

11. Page 6, line 136: The more appropriate reference for the delta-two-stream approach is Ritter and Geleyn (1992). [Ritter, B., and J.-F. Geleyn, 1992. A comprehensive radiation scheme for numerical weather prediction models with potential applications in climate simulations. Mon. Wea. Rev., 120, 303–325.]

We agree with the reviewer, thanks for pointing out this.

12. Page 6, line 142: Please note that the event of 14.11.2011 is not listed in Table 1.

Thanks for noticing this has been correctly indicated.

13. Page 5-6: If you are already running the 7km simulation, maybe you should consider to use the output and compare it to 2.8 km simulation to assess the benefit of high-resolution (and or switching off convection parametrization) simulations for that region.

The 7 km simulation has been run only in reference mode (CTRL). The benefit of the CCLM-2.8 km high-resolution convection permitting simulations versus CCLM-7 km with parameterized convection has been already investigated in detail in the past e.g. in Fosser et al. (2014). We additionally performed some comparisons, please see answer to comment number 5.

14. Page 6, line 153: To what soil texture do you put it? Which soil type from page 5 line 130?

The soil types are histosols, clay and loamy clay, visible in Figure 1 as they are bordering the Dead Sea.

15. Page 6, line 166-167: Can we talk about the trends in 10-yearlong simulations?

Modified

16. Page 7, line 173: This is how you should do the differences, but note that you do them with respect to the sensitivity simulation. See general comment 6.

Thank you for pointing out this, as previously explained we modified all calculations in the manuscript.

17. Page 7, line 196-199: I do not understand this paragraph.

This corresponds to the classification in Table 1, which has been clarified now in the text.

18. Page 5-8: I do not find any explanation on how do you define heavy events that you list in Table 1 or how do you classify them as localized or widespread.

The events were selected with an area mean difference in precipitation larger than 0.1 mm/d. localised or widespread nature of the vents was assessed visually for each case. Due to the low temporal resolution of the simulation it was not possible to use any predefined index such as tau, which requires more frequent outputs.

19. Page 9, line 251: For consistency, please use only one name for the sensitivity experiment.

Changed

20. Page 9, line 254: I still do not understand how do you define heavy precipitation events.

Please see 18

21. Page 9, line 256-257: You do not show that results, but could you at least mention how much is that difference? If it is not that significant or large, I do not see why you mention in the abstract that there is that difference.

Initially we included a table specifying the number of dry and wet days as well as the differences between simulations. However, since the differences where almost negligible, just a few days as described in the text, we considered this table was not relevant enough to be included in the final version. We agree with the reviewer that the differences are not significant, therefore we removed this information from the abstract.

22. Page 9, line 264: I do not see reduced precipitation in the SENCLIM experiment.

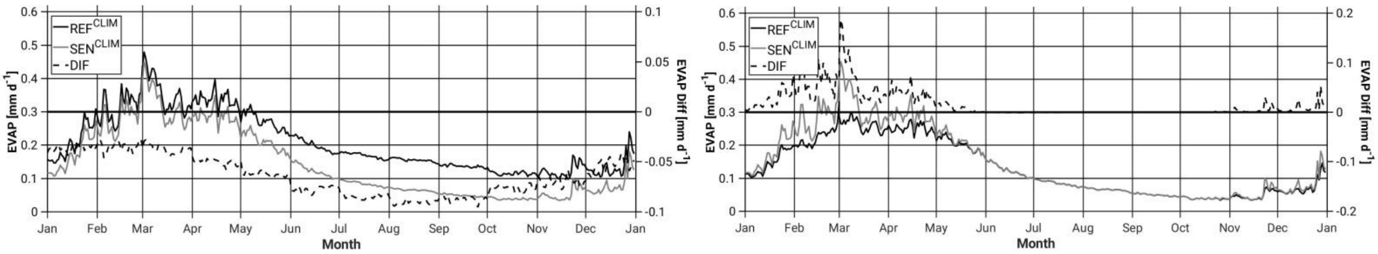
For each event the percentage of precipitation change has been calculated and included in the table. Additionally, the total percentage of change for the whole period, which corresponds to a reduction of about 0.5 % in the SEN simulation has been calculated and all this information has been included in the manuscript.

23. Page 10, line 280-281: How do you define these regions? This should be explained in the methods.

This information has been included in the methodology as suggested by the reviewer.

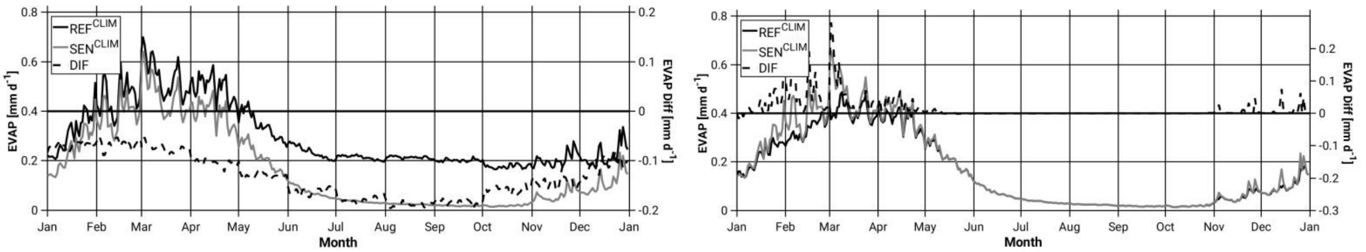
24. Page 10, line 289-295: Do you always use only land points or just for Figure 3? If just for Figure 3, explain why do you do it. How is that contributing to the overall analysis?

- *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.



25. Page 13, line 381-383: What is the relation between these two events? Are not they too close? Why only these two are chosen from the same period and with the same synoptic situation?

These two events where those showing the larger difference between the REF and SEN simulations (FIG SAL), the synop situation and the fact that they were close in time were no relevant factors for our analysis *rather the mechanisms responsible for the differences observed between both simulations. Even though the two cases were close in time and a connection was to be expected between the first and the second periods, after analysing atmospheric conditions between these two periods similar atmospheric conditions were found.*

26. Page 13, line 393: Caption below Figure 7 says that this is mean precipitation and not accumulated.

This has been corrected

27. Page 17, line 522-524: This is the third time that you mention these results, so it adds on their importance but still you do not show them in the manuscript. Either just mention it in the discussion, but if you want to discuss them in the abstract and conclusion you should consider adding these plots to the manuscript. Please note also that these differences could be larger for the hourly precipitation events i.e., more local convective events which would depend on the local evaporation sources.

As previously discussed, we agree with the reviewer, therefore we removed this in the abstract, and in the conclusion just mentioned that number of dry/wet days is not largely affected, suggesting that these differences could be larger for hourly precipitation events to point out the limitation in this study and a possible future aspect to be studied.