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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Near East Desertification: impact of Dead Sea drying on convective rainfall" *by* Samiro Khodayar and Johannes Hoerner

Anonymous Referee #1

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

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The paper presents an interesting idea of how removing Dead sea can impact the precipitation and evaporation in the surrounding areas. Thus, it addresses the important topic of drying Dead sea in a future warmer climate and how drying of the sea alone can impact the climate. Due to its importance, the paper could be considered for the publication but some major issues have to be addressed. I list them below.

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 analyze 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".

2. Why do you use high-resolution convection-permitting simulation since you analyze 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.

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.

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.

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.

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.

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.

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.

Specific comments:

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

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.

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

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

event?

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.

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.

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

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.

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?

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.

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.]

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

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.

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

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

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.

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

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.

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

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

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.

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

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

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?

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?

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

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.

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