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Interactive comment

Interactive comment on "Precipitation response to Aerosol-Radiation and Aerosol-Cloud Interactions in Regional Climate Simulations over Europe" *by* José María López-Romero et al.

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1 General Comments

This paper documents relatively long (20 years) simulations of precipitation with the regional WRF-Chem model driven by ERA20C reanalysis. Two simulations with interactive aerosols (one including only the aerosol direct radiative effect, another also the effect on cloud microphysics) are compared with a baseline simulation with fixed aerosols. It is found that the use of interactive aerosols decreases precipitation in Central/Eastern Europe and increases it in the Eastern Mediterranean. Detailed analyses

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regarding the number of days with precipitation with different thresholds are carried out.

The treatment of aerosols in regional climate models is often rather primitive, and therefore I think the authors have carried out a valuable set of experiments. At the same time, I cannot recommend the publication of this paper in ACP, unless substantial improvements are made in the analysis and reporting of the results. The reasons for my concerns are outlined below.

We strongly appreciate the positive view of the reviewer and acknowledge the time devoted to the revision the manuscript and the fruitful comments leading to the improvement of the manuscript.

2 Major comments

1. My primary concern regarding this paper is that while it documents in some detail how precipitation changes, the physical interpretation of the results is rather lacking. The paper fails to properly address the question, what are the physical mechanisms leading to these changes in precipitation. Only a few cursory statements are made in this respect. The changes in precipitation could be caused by several mechanisms. They could arise through the impact of aerosols on cloud microphysics, or their impact on surface temperature (which could suppress convection), or through changes in large-scale meteorology (although the latter are probably small due to the use of nudging in the outer model domain).

   The revised version of the manuscript extends the discussion on the causes behind the changes, together with the analysis of additional meteorological variables as temperature, radiation and three-dimensional fields (included as supplementary materials and discussed within the text). Additionally, the introduction has been extended in order to further include a description of the interactions leading to modifications in the precipitation regimes. Nonetheless, most of the studies on the current topic available in the scientific literature are case studies or ideal cases, for a certain type of cloud, type of aerosol or meteorological situation (see for example Khain et al (2007)). The aim of our work covers a climatic period and hence the separation of different circumstances is complex due to the internal variability of the model (the inner domain is large enough to generate it) and the mixture of aerosols and situations, in fact we obtain a important decrease of the temporal correlation among the experiments in some parts of the domain. Hence, the analysis presented in the manuscript focuses in statistical changes both in total precipitation and precipitation regimes. In the revised version of the manuscript we have deepened in the discussion of physical processes based on the available scientific literature and on the climate conditions of different European target areas.

2. To make it easier for the reader to interpret the findings, simulation results should be shown for additional physical quantities. It is very difficult to understand precipitation by looking at precipitation (and low clouds) alone. Most obviously, the paper should start with briefly showing how the aerosol fields (AOD, CCN, and aerosol radiative forcing, if available) differ from the baseline simulation, since these differences are the root cause for the changes in precipitation. I realize that some of this information is probably available in the cited papers by Palacios-Pena et al., but this paper should be able to stand alone âĂŤ it should not be the reader's task to hunt for necessary information in other papers. Furthermore, changes in surface temperature are potentially important for convection, and they are referred to at a couple of occasions, but it would be better to actually show them. Other quantities that should be checked (and possibly shown, if their changes seem important for precipitation) include meteorological fields like surface pressure, relative humidity and mid-troposphere vertical velocity.

As commented for Item 1, the revised version of the manuscript extends the discussion on the causes behind the changes, together with the analysis of additional meteorological variables as temperature, radiation and three-dimensional fields. Most of the fields represented are added as Supplementary Material in order not to modify largely the structure of the manuscript.

3. The interpretation of the results is also complicated by the fact that data for all seasons are lumped together. Yet, the processes generating precipitation, and potentially their sensitivity to aerosols, depend strongly on the season. Especially concerning central-eastern Europe, which shows the clearest signal in precipitation, convective precipitation dominates in summer, while stratiform precipitation associated with synoptic weather systems dominates in winter. I recommend that the authors first look at precipitation on a season-to-season basis (at least distinguishing between the warm and cold seasons), and then focus the detailed analysis on the season(s) with the most meaningful signals.

   In a preliminary analysis, the seasonal interpretation was conducted. However, the most significant signals where depicted for the entire year, probably due simply because of statistical issued. Therefore, in the original manuscript we decided to represent only the annual results. However, following the Reviewer's advice, we present the seasonal analysis of the results, focusing mainly in the analysis of the differences between the simulations. Those analysis are presented as Supplementary Material and the results discussed along the text in the revised version of the manuscript.

4. While the authors have conducted both ARI and ACI simulations, the ARI results are not discussed much, except for Fig. 6. I strongly recommend to show the ARI-BASE and ACI-ARI differences, at least for the time-average precipitation in Fig. 2. It is vital information for understanding to which extent the precipitation changes arise from aerosol direct and indirect effects.

The reviewer is absolutely right. For that, the revised version of the manuscript includes the ARI simulations in the panels of figures. The text has been modified accordingly in order to discuss the new results.

5. There are rather many issues with the use of English language. At the end of the review, I list cases which I found disturbing for correct understanding of the text. This is not intended to serve as a complete language check.

   We really appreciate your contribution to the improvement of the language. The final version of the manuscript will be revised by a native English speaker.

3 Detailed comments

1. line 16: should this be “eastern Mediterranean”?

   Yes. It has been corrected as suggested.

2. line 29: Can you add a reference to a publication listing the WCRP five major scientific challenges?

   We got this information from the web page of the World Climate Research Programme (WCRP). Checking it again we noted that that page has not been updated from a long time. We decided to remove this sentence from the manuscript.

3. line 32: I suggest replacing “The main tool” with “One of the main tools”. The IPCC AR5 estimates of aerosol radiative forcing use satellite observations to adjust model- based results.

   Changed as suggested.

4. lines 40–44: A key point of the convective invigoration mechanism of Rosenfeld et al. (2008) is that the slower cloud-droplet-to-rain conversion allows the droplets

to be transported above the freezing level, and therefore, the latent heat released in freezing makes the convection more intense.

Following the Reviewer's advice, this point has been added to the revised version of the manuscript in the introduction and also is used in the discussion.

5. lines 46–49: It would be useful to give a bit more information on the cited studies (e.g., which regions were considered?).

   We have incorporated further information about the state-of-the-art studies cited as well as some more new works, including area, aerosol type and size, etc..

6. lines 59–60: "and abundant number of cloud condensation nuclei (CCN) (Forkel et al., 2015) high enough for clouds to form without this variable being a limited factor". In fact, the lack of CCN is almost never a limiting factor for cloud formation (this could perhaps happen in remote marine locations in very specific conditions). However, a low CCN value may result in clouds that precipitate more readily, which can reduce the cloud lifetime and therefore the average cloud fraction.

   Thanks for your comment. We have incorporated it to the revised version of the manuscript.

7. line 67: "black anthropogenic aerosols". Do you mean black carbon, or absorbing anthropogenic aerosols in more general? Furthermore, this paragraph gives the impression that anthropogenic aerosols cause warming and natural aerosols cause cooling, which is misleading. Many anthropogenic aerosols, most prominently sulfates, are largely non-absorbing, so the total effect of anthropogenic aerosols is probably one of radiative cooling.

   The reviewer is right. We refer to black carbon as it is mainly generated by anthropogenic activity. We have clarified this point in the new version of the manuscript.

8. lines 112, 116: You mention the use of both the Goddard shortwave radiation scheme and the RRTMG scheme. To my knowledge, these are different radiation schemes. Please explain.

   The reviewer is right and the information was mistaken. We used the RRTMG scheme. This correction has been incorporated in the revised version of the manuscript.

9. lines 127-129: While AOD (it should be “aerosol optical depth”) has been evaluated by Palacios-Pena et al. (2020), it would be definitely good to show the time-mean AOD fields also in this paper (see major comment 1).

   AOD fields and the differences among the experiments has been included in the revised version of the manuscript as supplementary material.

10. line 163: correlation matrix of what?

    The correlation matrix of the constructed series for each point. The constructed series are the differences between the number of days of precipitation for several thresholds. The sentence has been revised accordingly for the sake of clarity.

11. 11. lines 174-179: The spatial redistribution of precipitation is interesting, but is very difficult to figure out why it is happening, based on the information given in this paper. Please see the major comments 1-3.

    In order to provide clearer information the new figures included in the manuscript are presented and a deeper discussion is included.

12. 12. line 193: “(not shown)”. In fact, you do show the differences between ACI and BASE in Fig. 2.

    We shown ACI-BASE but we do not present ARI-BASE. Anyway, new figure 2 of the manuscript is shown, and the differences commented.

13. 13. line 214: According to Fig. 3b, the correlation coefficient in 0.78, not 0.40.

    We made a mistake here. We mean "In the case of PM10 .... This paragraph has been rewritten including the 0.78 value for AOD and 0.4 for PM10.

14. 14. lines 215–216: The more strongly negative ACI-BASE precipitation differences in Central Europe associated with high PMratio events are a curious result. Why is the ratio of PM2.5/PM10 more important than PM2.5 alone? In general, at least in this region, I would expect that particles with diameter $<$ 2.5 μm are much more important than larger particles, especially for CCN and usually also for the aerosol direct radiative effects, because of their much larger number concentration. A somewhat remote possibility is that this result is related to giant aerosols enhancing precipitation, and thereby opposing the effect of smaller aerosols (this could be checked by looking at events defined wrt. to the difference PM10-PM2.5). Another possibility is that the result is coincidental, that is, more related to the different meteorological conditions associated with high vs. low values of the PMratio, rather than to the impact of aerosols on cloud microphysics. This risk is enhanced by the fact that all seasons, with different precipitation formation mechanisms, are lumped together.

    We really appreciate this comment. We have been revising some papers about the role of Giant Aerosols by Feingold et al. and this could be key point that helps us to improve our explanation on the decrease of precipitation (amount and number of days) in that area as well as the increase in the eastern Mediterranean. Some more disscusion will be added to the new version of the manuscript about these processes.

15. lines 217–220: Why would the greater amount of small particles lead to reduced low cloudiness? Note that according to Fig. 6(d,e), the reduction in low clouds seems to be related mostly to the aerosol direct (and possibly semidirect) radiative effects rather than their effect on cloud microphysics.

We agree with the reviewer that the reduction in low clouds is related to the aerosol direct and semidirect radiative effects. But, the reduction of low clouds in ACI is larger than in ARI, therefore the role of microphysics could be important. In fact, an analysis performed similar to the one presented in Figure 3 shows how that this relationship exists. Anyway, we understand that our explanation is not complete. As mentioned before, some more plots including ARI experiment results have been added, as well as a much more extended explanation linking the reduction/increase of low clouds and precipitation based on both experiments (ARI and ACI) to direct, semidirect, and indirect effects.

16. 16. line 236: "(significant differences)". Please refer to Fig. 2b to make it easier for the reader.

    The reference to figure has been added.

17. lines 237-240, 248-249: Given the very spatially scattered distribution of Region 3, it is hard to believe that this cluster really represents physically meaningful results, in spite of the apparent statistical significance. It seems more likely that the cluster analysis has just picked separately a group of points with increased frequency of large precipitation amounts, even if this increase itself might be caused by internal climate variability (i.e., be random). Note that grid points belonging to Region 3 are often neighboured by grid points belonging to Regions in which the frequency of heavy precipitation actually decreases.

    We agree that Region 3 has no spatial structure. We perfectly understand the doubts of the reviewer about the physical meaning of this region. However, from the statistical point of view, we obtain that a important portion of grid cells presents an coherent increase of moderate and intense precipitation events. At the same time, we can found in the literature that this increase is supported by some physical processes. We think that it is important to keep the message, but at the same time to warm the reader about the need of deeper studies about that, since it could be an artifact of the statistical methodology used.. We have rewritten the description of the behaviour of Region 3 trying to send the above message.

18. lines 251–262: You should consider the statistical significance of the differences also in the case of Fig. 6. Some of the details discussed in this paragraph might not be robust.

    As commented before, we have now included all plots showing ACI and ARI experimented. In addition we have add the statistical significance as in figure 2, and when discussing the results we take into account the statistical significance.

19. line 270: “Zone 5” should be “Zone 4” (or “Region 4”).

    It has been fixed up.

20. 20. lines 304-305. It is not clear to what this sentence refers to. Please explain better, or remove.

    We have removed that sentence. It do not provide any important message.

21. Fig. 2: Note that in statistical testing, one should be aware of the risk of false positives. If a test is conducted at the significance level p=0.05, on average 5% of grid points will show “significant” differences, even if the differences between the two fields are actually random. It would be good to compute the fraction of significant differences and show it e.g. in the figure titles (it seems not to be much larger than 5% visually?). A more rigorous technique for looking at this would be “controlling the false discovery rate”, see Wilks et al. (2016): Wilks, D.S., 2016: “The stippling shows statistically significant grid points”: How research results are routinely overstated and overinterpreted, and what to do about it. Bull. Amer. Meteor. Soc., 97, 2263–2273, https://doi.org/10.1175/BAMS-D-15- 00267.1.

We really appreciate the suggestion of the reviewer. We have calculated the fraction of significant differences and we refer them along the text. Anyway, we fix our attention on *significant areas* (group of nearby significant points) that are far of being false positive as stated in Wilks et al 2016.

22. Consider marking the statistically significant differences also in Fig. 6.

    Thanks for your advice. As mentioned above, all maps of differences show the statistical significance.

4 Technical and language corrections

1. line 9: do you mean “time-mean spatially averaged”?

   Yes. Corrected.

2. line 11: this should be “precipitation intensity regimes”.

   Corrected.

3. line 69: “dispersion” probably refers to “scattering”?

   Yes. Corrected

4. lines 73, 282, 285, 302 and 310. The use of “color” for describing clouds or aerosols is not clear, and certainly not standard scientific terminology. In the present context, “optical properties” would perhaps be the best term; for aerosols, “refractive index” could also be used.

   We acknowledge your suggestion, now we use *optical properties*

5. line 159: replace “on a non-regular basis“ with “in a non-linear scale”.

   Done.

6. line 256: add “causes” before “a reduction”.

   Done.

7. lines 277-279: The last sentence of Section 3 is not clear. Do you mean that in high PM10 conditions, clouds are preferentially located in the southern part of the area?

   We mean that high load of PM10 are usually associated with synoptical conditions that transport the PM10 (dust) from the south. We have rewritten the sentence in order to make it clearer.

8. line 302: replace “order of magnitude ...” with “quantitatively this improvement is small”.

   Done

9. line 310: replace “competence of CCN” with “efficiency of CCN”.

   Done

10. In Figure 3, it is impossible to see black numbers plotted on black or dark blue background. Also, the units of the color bar should be % (not “score”) in panels (c) and (d).

    Done

11. Caption of Fig. 4. The series used as the basis of the cluster analysis are not “time series” (in a time series, you would have time on the x-axis; here you have the precipitation threshold).

    Rigth. Now the caption reads .." Cluster analysis of rainy days: each color depicts a cluster with a different behavior of the ACI-BASE difference in number of days of precipitation over a threshold ...... "..

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12. In Fig. 5, “Zona” is Spanish. “Zone” or “Region” would be English.
    Fixed