Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-381-RC1, 2020

Atmospheric Chemistry and Physics
Discussions

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.

Anonymous Referee #1

Received and published: 7 June 2020

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 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.

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 meterology (although the latter are probably small due to the use of nudging in the outer model domain).

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 precipation. 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 — 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 vecolity.

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 distuinguishing between the warm and cold seasons), and then focus the detailed analysis on the season(s) with the most meaningful signals.

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.

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.

Detailed comments

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

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

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.

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.

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

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.

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.

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.

9. lines 127-129: While AOD (it should be "aerosol optical deth") 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).

10. line 163: correlation matrix of what?

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.

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

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

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 $< 2.5\,\mu$m 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 as-

sociated 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.

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.

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

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.

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.

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

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

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*.

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

Technical and language corrections

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

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

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

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.

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

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

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?

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

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

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).

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).

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

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-381, 2020.