General comments

I thank the authors for their substantial efforts in revising the manuscript, and for the additional material included.

Based on this material, it is clear that there are multiple reasons for the differences in precipitation between the different experiments. These include not only the "straightforward" effects of ARI and ACI, but also changes in atmospheric circulation (whether caused by the different treatment of aerosols, internal variability, or their combination). So it is understandable that in many cases the physical reasons for the precipitation differences remain unclear. Some of the physical explanations provided by the authors seem reasonable, while some appear uncertain or even unlikely. A few such instances are pointed out in the specific comments below. In addition, there are rather many minor technical/editorial issues that should be corrected to make the paper easier to read. The current manuscript gives an impression that you did not have quite enough time for a proper proof-reading in the end.

Note: The comments and line numbers in this review are based on the manuscript version "acp-2020-381-manuscript-version3.pdf". This version seems to differ in some details from the marked-up manuscript provided as part of the author response.

Specific comments

1. line 45: Please indicate the region studied (the title of the Da Silva et al. (2018) paper suggests it was the Euro-Mediterranean region).

2. line 60: I suggest deleting "high enough for clouds to form without this variable being a limiting factor".

3. line 185: For completeness, also mention the ARI−BASE differences in spatially-averaged total precipitation. Presumably small?

4. lines 198–199: Replace "being stronger in winter" with "the absolute changes being largest in winter"? (This is not true for the relative changes shown in Figs. 7 and Fig. 9 in the Supplementary material).

5. line 201: presumably, this should be "decrease of clouds".

6. lines 202 and 203: “changes in temperature are opposite for tasmax and tasmin ...”. Are they? Based on Figs. 14 and 15 in the Supplementary material, the spatial correlation between the changes in maximum and minimum temperatures could even be positive.

7. lines 251–257: If I understand the logic of the reasoning here correctly, it is suggested that a greater concentration of small particles acts to reduce low clouds and precipitation due to semi-direct effects (i.e., black carbon aerosols). However, it seems rather treacherous to draw such a conclusion based on the PMratio alone. The mass fraction of aerosols below 2.5 μm diameter does not necessarily tell much about BC. Since BC fields are available in the model (Supplementary Fig. 5), you could check this hypothesis more directly by adding an analysis wrt. “BC events” in Fig. 4.

Note that other explanations are also possible. For example, it could also be that cases with a lot of small particles happen to be associated atmospheric circulation types with drier-than-average atmospheric conditions in this region (obviously, this is speculation too).

8. line 301: “... higher concetration of small particles that modifies the properties of clouds, inhibiting precipitation processes again by reducting clouds due to microphysics processes, since [in] this area there is a prevalence of small aerosols”. Again, it is not obvious that the prevalence of small aerosols should lead to reduced low cloudiness (what would be the microphysical process causing this?). Rather this looks like a case where some factor X (possibly changes in atmospheric circulation) reduces both cloudiness and precipitation. Perhaps it would be better to say that the explanation is not clear, rather than guess.

9. lines 306–307. “While small particles inhibit the formation of clouds by semidirect effects, larger aerosols ease the cloud formation and precipitation by indirect effects”? Referring to comment 8, the role of semidirect effects could be better checked by looking at the BC concentration rather than PM2.5 or the PMratio. The PMratio does not tell much about BC. It is also not clear why larger aerosol ease the cloud formation (the possible role of giant aerosols on precipitation is another matter). Note that the lower size limit for CCN is around 0.1 μm. So if you draw the line between “small” and “large” aerosol particles at a 2.5 μm diameter, then the vast majority of CCN are “small’. Perhaps this paragraph could be deleted altogether?

10. lines 370–371: “Our experiments show a extra depletion of cloudiness, probably related to a faster evaporation of drops”. Replace “probably” with “possibly”? Very little actual evidence for this has been shown in the paper.

11. lines 377–386: Would it be worth adding something like “It would be interesting to see to which extent other regional models would reproduce the current results for the Euro-CORDEX region”? As a friendly reminder of the possible (and probable) model-dependence of the results...

12. Fig. 7: I think it would be most logical to show this figure already in the early part of the paper, i.e. between Figs. 1 and 2.

Technical corrections

1. line 125: replace “departing” with “originating”.

2. line 136: Mention the figure numbers in the Supplementary material to make it easier for the reader to locate these figures. Also in other instances where Supplementary material is referred to (although perhaps not on lines 151–154).

3. line 163: Add parenthesis around ARCI−BASE.

4. line 186: The text refers to Fig. 2a, but the figure panels are not identified by letters in Fig. 2. Please also check for other instances like this in the text.

5. lines 221 and 251: Figure number is missing.

6. lines 264–290, and Figs. 6 and 8. Different terms are used in the text (“Region”) and in the figures (“Cluster” in Fig. 6, “Zone” in Fig. 8). A consistent notation would be preferable — I would vote for “Cluster”, since this is based on cluster analysis.

7. line 295: Figure 4 should be Figure 2?

8. line 304: Why “whole” ARI effect?

9. line 308: Should “Figure 8” be “Figure 7”?

10. line 312: “Figure 7” should be “Figure 8”.

11. line 368 and 370: “CNN” shoud be “CCN”

12. Figure 2: In the title of the middle column, “BAS” should be “BASE”.

13. Figure 3: Panel titles would be needed here. Now it is not clear which quantities are shown. Also, mention in the caption that the squares refer to significant differences *between the ACRI and BASE experiments*.

14. Caption of Fig. 4. Replace “non-constant linear behavior” with “non-linear behavior”.

15. Fig. 5: The cluster numbers 1-5 should be shown.

16. Caption of Fig. 7: Please correct the units for PM10.

17. Figure 8: Please indicate which experiments are compared here.

18. In the Supplementary material, please name figures as S1, S2 and so on (note that the remaining comments use this notation).

19. Regarding the 3rd column in the figures in Supplementary material, the figure captions should indicate whether the absolute or relative differences are shown. This seems to vary, and now the reader has to figure it out on a case-by-case basis.

20. In Fig. S1, the title of the 3rd column should be “ARCI−ARI”.

21. In Figs. S6–S22, replace “BAS” with “BASE”.

22. Caption of Fig. S4: replace “UNIDADES” with “unitless”.

23. Caption of Fig. S5. What is “BC2”? At which height is it defined?

24. Fig. S8: Are these results really given in “number of days per month” as the caption indicates? The numerical values (up to 10–20) seem very large.

25. Caption of Fig. S10: What are the units? Percentage points?

26. Caption of Fig. S17: The numerical values seem to be in units of “Pa”, not “mb” as the caption states.

27. Figures S21 and S22: Please check the units. Temperature differences of several K seem very large (and much larger than those at the surface level in Figs. S14 and S15).

28. Please have the language checked. I noted rather many issues with the English language, especially in the new parts of the manuscript.