

Interactive comment on “Gains and losses in surface solar radiation with dynamic aerosols in regional climate simulations for Europe” by Sonia Jerez et al.

Anonymous Referee #3

Received and published: 28 September 2020

This manuscript, submitted to Geoscientific Model Development, aims to identify the role of interactively modeling aerosol in regional climate simulations over Europe, by conducting a sensitivity study with the WRF model. The focus is on solar radiation at the surface during summer. Both a present and a future period are considered. Changes in cloudiness are presented as the main driver of the changes in solar radiation. There are some interesting features in this study, such as long simulations with the WRF-Chem model using interactive aerosol that are computationally demanding. However, I believe that the main problem is that the aim of the study is not actually addressed. I believe that separating the “interactive” part of aerosol modeling and making general comments about it is not possible in the current study. Thus it is a problem of

C1

methodology and structuring of the whole manuscript. Moreover I believe that a significant clarification is need in the current methodology regarding the BASE simulation that is the basis for comparison. I would hesitate to recommend it for publication in its current form. However, I believe that it could stand as a sensitivity study aiming to describe the impact of the specific model and aerosol treatments used. I would suggest major revisions regarding: the aims of the study, including a validation, possibly changing the analysis under clear-sky conditions, clarifying the aerosol treatment in the simulations. In the end, I think the study could provide some interesting points to the community.

Major comments:

1. One of my major concerns is that the nature of the BASE experiment is not clear to me. It is stated that it works with a specific aerosol concentration and that “the aerosol radiative effect is assumed to come as an external forcing.” I am not sure what this means. Does the BASE experiment let these aerosols interact with radiation? In this case the AOD field needs to be shown. Or their only impact is that they are just used by the microphysics to facilitate cloud formation? In any case, the nature of aerosol in the BASE experiment needs to be clearly stated so that the reader understands the results of the comparison. Moreover if BASE has an AOD that interacts with radiation, how much does it differ from the AOD of ARI and ACI? Are the differences between BASE and these simulations attributed to the difference in AOD and not to the introduction of dynamic aerosol?

2. It is very interesting to try and identify the impact of interactively modeled aerosols. However, I am not sure that this is achieved in the study. You can make a statement that, for example, the ARI experiment that uses “this specific” interactive aerosol treatment in WRF-Chem has “this specific impact” on radiation. This statement could be useful to the community as a sensitivity study of the model and aerosol scheme. However, I do not think that you can attribute this impact only to the “interactive” part. Probably, a first step towards that direction would be to have additional experiments

C2

enabling aerosol-radiation and cloud interactions using static aerosol fields with the same mean AOD as the ones in ARI and ACI.

3. I believe a validation (even a quick one) of the simulations, especially regarding rsds and AOD, should be part of the study in order to assert that they do capture the basic patterns of the examined variables. I do understand that they are compared against the GCM (and that the GCM has been probably validated), but still a validation would make the results more robust.

4. The methodology to calculate Clear sky conditions was a bit unusual to me. I am aware that the radiation code in WRF (and I think this is the case for version 3.6.1) provides the clear-sky radiation at every time step simultaneously with rsds. It would probably be better to use that feature. I also have a question regarding the methodology. It is stated (page 6, 150-152) that in order to consider a specific grid point in the analysis you need to have at least 15 records per period that are not missing values. Ok so far. It is stated (page 6, lines 153-154) that "(which, according to our methodology, would occur only if all days within a summer season have CCT values >1%)." So, if I understand correctly even if one day within a summer season has a CCT value <1, that summer season gains a valid value based only on that day and is considered in the analysis?

5. The use of no time evolving anthropogenic aerosol in the future period by ARI and ACI experiments is not ideal. It is good that this deficiency is stated in the manuscript (page 8, line 218). Moreover, it would be interesting to see what are the rsds differences between the GCM and ARI/ACI for the future period.

Minor comments:

-Page 1, line 20 "reduction about 5% in RSDS was found when aerosols are dynamically solved". This is compared to BASE? It must be clearly stated.

-Page 2, line 33 The phrase "all about cumulus" I believe should be clarified a bit better.

C3

Is this about convective phenomena, the cloud fraction scheme or both?

-Page 4 lines 97-98. In the BASE experiment "the by-default WRF setup was used, which considers 250 cloud condensation nuclei per cm³ to form clouds". I think the term "by-defalut" might be a bit misleading. I understand that this concentration of CCN is probably related to the Lin microphysics scheme used in the experiments and this should be stated.

-I do not understand how ACI (page 5, lines139-141) works. What is meant by "Although this WRF-Chem version (3.6.1) does not allow a full coupling with aerosol-cloud interactions. . ." I believe it should be clearly stated which are the parts of the aerosol-clouds interactions that are missing. Also I think it should be stated to which variables the single and double moment treatment is applied.

-I believe it is useful to know which statistical test is used (t-set, non parametric Mann-Whitney. . .) to determine statistical significance.

-Total cloud cover values over southern Europe in summer are usually small. Thus, the changes in CCT between the experiments could be in some cases negligible but the relative (percentage) change could be inflated. I believe this should be stated in the manuscript. Also, it would be interesting to see a plot with the plain difference in CCT between experiments in the supplement.

-Page 7, lines 185-186. "Contrary, the effect of interactive aerosols schemes. . ." The way it is written gives the impression that the authors are talking about interactive schemes in general. I think it would be better to avoid generalizing the results of this specific sensitivity study.

-Page 8, lines 209-210. "These latter are more widespread in ARI than in BASE, which makes the ARI pattern the most similar to the change pattern from the GCM". I do not clearly see this in Figure3.

Technical corrections:

C4

Page 7 line 183 “varables” -> variables

Page 7, line 188 I am not aware of the word “devanishes”. Could this be a spelling mistake?

Page 10, line 274 experimts -> experiments

Page 1, line25 much more softer -> much softer

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-238>, 2020.