Anonymous Referee #3
Authors' response

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 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.

We do thank the reviewer for the time devoted to read and thoughtfully comment on our work. Below we provide detailed answers to each comment, hoping to have been clear enough in our explanations. Attending these comments and the ones posted by the other reviewers and the Editor, the new version of the manuscript:

1 – Has been entirely revised by a native speaker in order to improve the redaction.

2 – Has a new title. The change intends to avoid that the reader interprets that we are comparing simulations with dynamic vs. static aerosols. The new title is:

"Sensitivity of surface solar radiation to aerosol-radiation and aerosol-cloud interactions over Europe in WRFv3.6.1 climatic runs with fully interactive aerosols"

In this line, we have made an effort to make the scientific purpose of the manuscript clearer throughout the whole text.

3 – Includes further details and arguments on the experimental set-up and the methodology. Section 2 has been divided into 3 subsections.

4 – The formerly labeled as ACI simulations are now named ARCI to emphasize that these include both aerosol-radiation and aerosol-clouds interactions.

5 – Includes a brief validation exercise. We now face the outputs of our simulations with ERA5.

6 – Includes two subsections within the Results section: one for the historical simulations and another for the future projections.

7 – Includes a deeper discussion of the results attending several comments by the reviewers, e.g.:

- the activation of the autoconversion scheme in the ACI simulations hampers a direct attribution of the signals to the aerosol-cloud interactions from a physical point of view (the attribution can be made, from a modeling point of view, to the activation of these interactions in the model);

- the fact that we kept constant the anthropogenic aerosol emissions in future simulations permits to better isolate the signals from the aerosol-radiation-cloud interactions due to the so-called climate change penalty alone, while reduces the reliability of the future projections obtained;

- the signals obtained for different seasons (additional analysis is provided as Supplementary Material);

8 – Includes a link where all the data and codes to reproduce our study have been made publicly available: http://doi.org/10.23728/b2share.682b1c6311134b36a18f59a99a443afd.

We are confident that these major changes have improved significantly the manuscript and provides a larger support to its key findings.

We must also notice that we used wrong AOD values in the previous version of the paper, as it was noted by the reviewer2. These had been computed from the TAUAER3 and TAUAER4 variables, which do exhibit a weird evolution along the year. After inspection, we figured out that these and the EXTCOF55 variables had been wrongly recorded in the wrfout files (not new, apparently, see e.g.: https://forum.mmm.ucar.edu/phpBB3/viewtopic.php?t=9313&p=17464). So we have now adopted an alternative method to compute AOD following Palacios-Peña et al (2020), where, in fact, the representation of AOD by these model configurations (ARI and ARCI) were deeply evaluated. The new AOD files were estimated using the reconstructed mass-extinction method (Malm et al 1994) from the well-recorded concentrations of the various aerosol species in the wrfout files, namely: black carbon, organic carbon, dust and sea salt. Sulfates were estimated from SO2 and OH recorded concentrations using the same kinetic reaction as the one implemented in the RACM-KPP module. We want to remark that the mistake occurered during the postprocessing of the wrfout files, while WRF-Chem run satisfactorily. These wrfout files were removed after postprocessed, so we have now generated a sample one (using the ARI configuration) and uploaded it for checking together with all the other data files. Importantly, this change in the methodology for estimating AOD values did not alter the the overall results of the paper.

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?

We agree that the BASE experiment was poorly described. Now we say:

“BASE: aerosols are not considered in the simulations. No aerosol climatology is used and no aerosol interactions are taken into account by the model. WRF-alone considers a constant number of cloud condensation nuclei (250 per cm^3, set in the model by default) to enable the formation of clouds.”

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

We also agree here. We were wrongly giving the message that signals were due to the interactive aerosols modeling approach adopted here as compared to a more conservative (and common) approach based on non-interactive aerosols, which is something that we did not inspect. We have accordingly reformulated the title and redaction of the manuscript.

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.

The manuscript now includes a brief comparison of the present-day simulations with ERA5 (for RSDS). The representation of AOD by these model configurations (ARI and ARCI) were deeply evaluated in Palacios-Peña et al. (2020). Nonetheless, find attached here a figure with the AOD climatologies from ARI and ARCI and those from MACv2.

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 CTT 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?

Unfortunately, we did not save clear-sky values from the model outputs, so we needed to adopt an alternative methodology.

Regarding the methodology, the reviewer’s interpretation is right. In any case, we show seasonal climatological values (or differences), thus the results are independent of that (we simply average over all days with CCT<1%). The number of seasons with non-missing values just affects the interannual variability of the seasonal series, thus playing a role when assessing the statistical significance of the differences between that climatological values. Therefore, outliers values (in case) should affect very little the overall results.

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.

As stated above, this approach permits to better isolate the signals from the aerosol-radiation-cloud interactions due to the so-called climate change penalty alone, while reduces the reliability of the future projections obtained, in fact. We now we make more emphasis on this point in the manuscript.

Attached a figure with ARI-GCM and ARCI-GCM differences in rsds in the future period. Not that different to those in the present 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.

Done.

-Page 2, line 33 The phrase "all about cumulus" I believe should be clarified a bit better. Is this about convective phenomena, the cloud fraction scheme or both?

It is more correct to say "convective phenomena" indeed. Amended.

-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.

This part has been reformulated. Anyway, this CCN value (250 per cm3) is not linked to the microphysics scheme, but something more general in the model.

-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.

We have amended the lack of description of the WRF setup used to perform the simulations labeled as ARCI by including the follwoing in the text (in section 2):

"Aerosol-cloud interactions were implemented by linking the simulated cloud droplet number with the microphysics schemes (Chapman et al 2009) affecting both the calculated droplet mean radius and the cloud optical depth. Although this WRF-Chem version (3.6.1) does not allow a full coupling with aerosol-cloud interactions, the microphysics implemented here is a single moment scheme that turns into a two moments scheme in the simulations denoted as ARCI. One-moment microphysical schemes are unsuitable for assessing the aerosol-clouds interactions as they only predicts the mass of cloud droplets and does not represent the number or concentration of cloud droplets (Li et al. 2008). The prediction of two moments provides a more robust treatment of the particle size distributions, which is key for computing the microphysical process rates and cloud/precipitation evolution. In this sense, although the Lin microphysics is presented as a single moment scheme, the

WRF-Chem model allows to transform the single into a double moment scheme. A prognostic treatment of cloud droplet number was added (Ghan et al. 1997), which treats water vapour and cloud water, rain, cloud ice, snow, and graupel. The autoconversion of cloud droplets to rain droplets depends on droplet number (Liu et al. 2005). Droplet-number nucleation and (complete) evaporation rates correspond to the aerosol activation and resuspension rates. Ice nuclei based on predicted particulates are not treated. However, ice clouds are included via the prescribed ice nuclei distribution following the Lin scheme. Finally, the interactions of clouds and incoming solar radiation have been implemented by linking simulated cloud droplet number with the Goddard shortwave radiation scheme, representing the first indirect effect, and with Lin microphysics, which represents the second indirect effect (Skamarock et al. 2008). Therefore, droplet number will affect both the calculated droplet mean radius and cloud optical depth."

References:

Ghan, S. J., Leung, L. R., Easter, R. C., & Abdul Razzak, H. (1997). Prediction of cloud droplet number in a general circulation model. *Journal of Geophysical Research: Atmospheres*, *102*(D18), 21777-21794.

Li, G., Wang, Y., & Zhang, R. (2008). Implementation of a two moment bulk microphysics scheme to the WRF model to investigate aerosol cloud interaction. *Journal of Geophysical Research: Atmospheres*, *113*(D15).

Lin, Y. L., Farley, R. D., & Orville, H. D. (1983). Bulk parameterization of the snow field in a cloud model. Journal of Climate and Applied Meteorology, 22(6), 1065-1092.

Liu, Y., Daum, P. H., & McGraw, R. L. (2005). Size truncation effect, threshold behavior, and a new type of autoconversion parameterization. *Geophysical research letters*, *32*(11).

Mitchell, D. L., Rasch, P., Ivanova, D., McFarquhar, G., & Nousiainen, T. (2008). Impact of small ice crystal assumptions on ice sedimentation rates in cirrus clouds and GCM simulations. *Geophysical research letters*, *35*(9).

Skamarock, W.C.; Klemp, J.B.; Dudhia, J.; Gill, D.O.; Barker, D.M.; Wang, W.; Powers, J.G. A Description of the Advanced Research WRF Version 3; Technical Report NCAR Tech. Note TN-475+STR; NCAR: Boulder, CO, USA, 2008.

Tao, W. K., Simpson, J., & McCumber, M. (1989). An ice-water saturation adjustment. *Monthly Weather Review*, *117*(1), 231-235.

We have also acknowledged in Discussion the following:

"In the ARCI simulations, the autoconversion scheme called so that cloud droplets can turn into rain droplets is different to the autoconversion scheme activated in the ARI simulations. This change in the WRF-Chem configuration can lead to ARCI-ARI differences that do not come necessarily from the of the aerosol-cloud interactions from a physical point of view (Liu et al 2005). In fact, the activation of the aerosol-cloud interactions requires further changes in the model configuration (as compared to the configuration used for the simulations labeled as ARI) beyond the autoconversion scheme, such as the activation of aqueous chemistry processes, that could also have an added impact to effect that can be strictly attributed to the aerosol-cloud interactions. However, technically, the encoding of WRF-Chem model hampers to better isolate the effect of the aerosol-cloud interactions. Therefore, ARCI-ARI differences can not be attributed to the aerosol-cloud interactions from a purely physical point of view, but to the activation of the aerosol-cloud

interactions from a modeling point of view, since the autoconversion schemes necessarily change between ARI and ARCI. This should be beared in mind when interpreting the signals."

Reference:

Liu, Y., Daum, P. H., & McGraw, R. L. (2005). Size truncation effect, threshold behavior, and a new type of autoconversion parameterization. *Geophysical research letters*, *32*(11).

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

We used the t-test. Section 2 has now been splitted into several subsections. The last one includes more methodological details, as this one.

-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.

Figures 1 to 4 have been replicated to show plain differences. These new figures have been included as Supp. Material and used to describe the results.

-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.

The reviewer is right. We have followed this suggestion all along the revised manuscript.

-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.

The ARI pattern (Fig 3 c) shows the most widespreaded positive signals south-eastward, and the lowest negative signals northward.

Technical corrections:

Page 7 line 183 "varables" -> variables

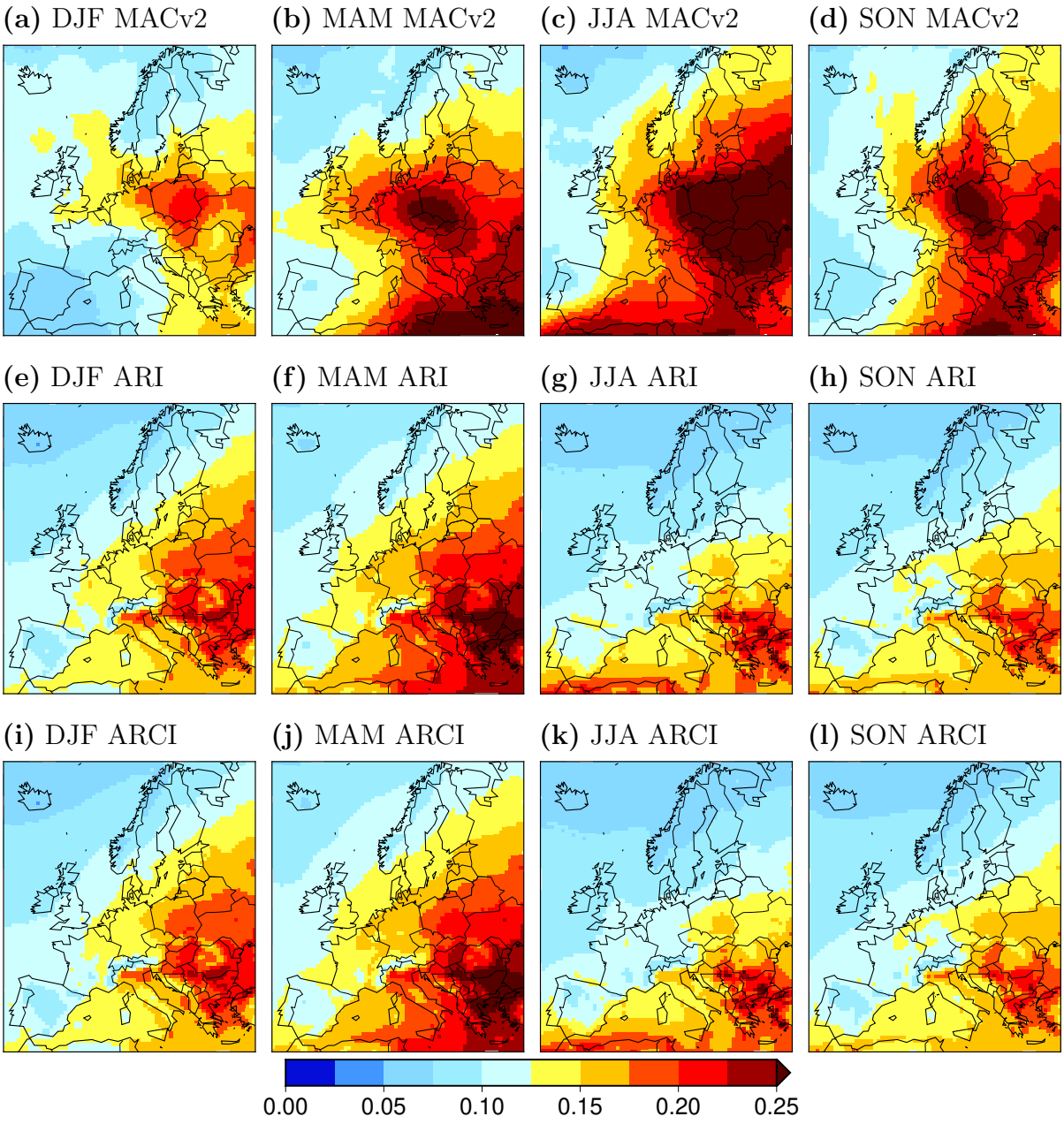
Page 7, line 188 I am not aware of the word "devanishes". Could this be a spelling mistake?

Page 10, line 274 experimts -> experiments

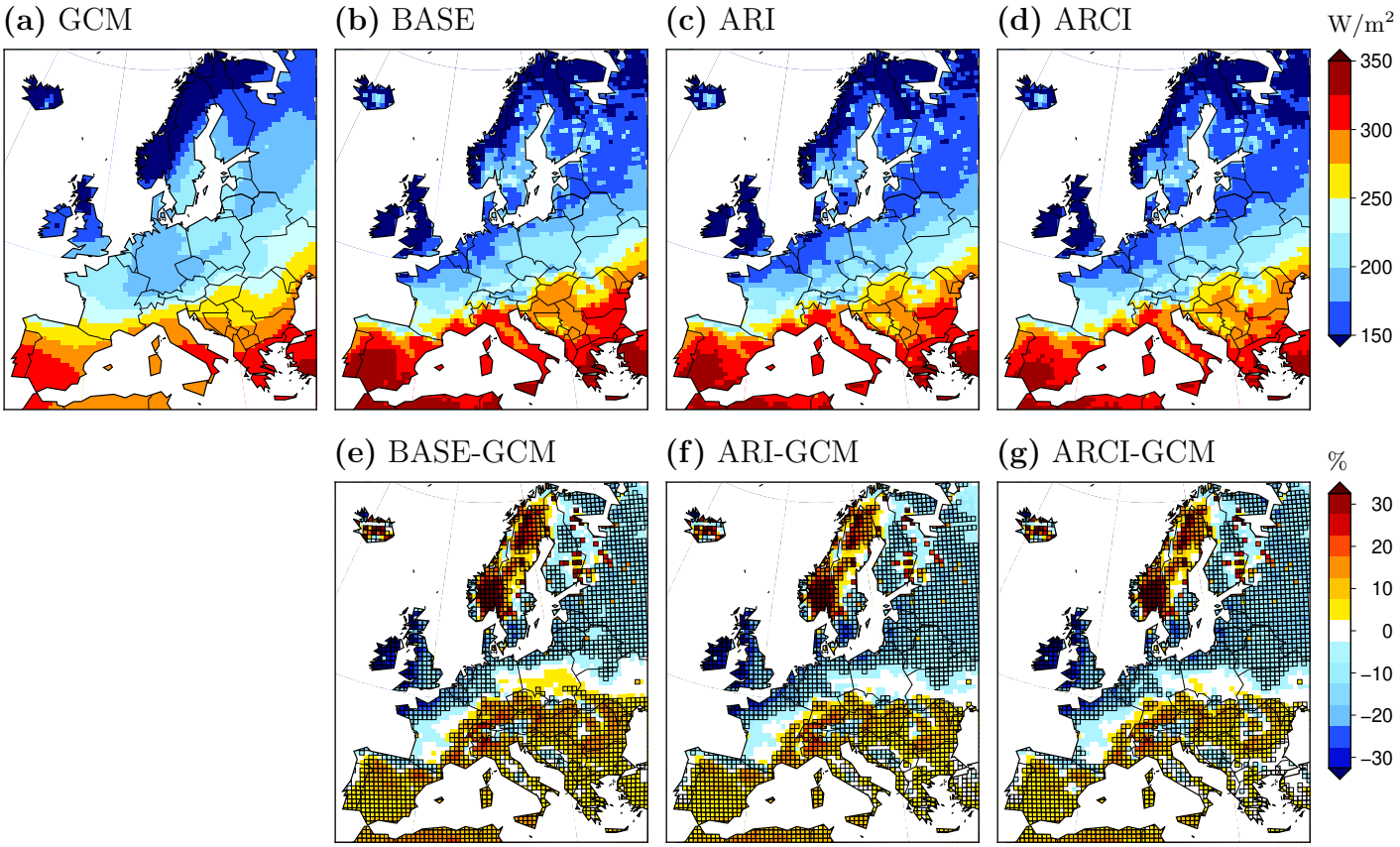
Page 1, line25 much more softer -> much softer

Thanks for these corrections. The entire manuscript has been revised by a native speaker.

AOD climatologies for 1991-2010



RSDS JJA climatologies for 2031-2050



RSDS summer climatologies in the future period from the GCM (a) and the WRF simulations (b to d); units: W/m2. Panels e to g depict relative differences between each WRF simulation and the GCM, squared if statistically significant ($p<0.05$); units: %.