Anonymous Referee #1
Authors' response

This paper quantifies a present day and future reduction in summertime solar radiation at the surface due to aerosol and aerosol-cloud interactions over Europe using WRF as a regional climate model (RCM). Previous work has used static aerosol concentrations to quantify insolation reductions due to aerosol, while this study uses online dynamic aerosols through the GOCART module in WRF-Chem.

Overall, this paper is missing interpretations of key physical processes, model validation, may have a flawed model design, and does not fall under the purview of GMD. As the paper currently stands, my recommendation is "reject." To enter major-revisions territory, significant changes to the model setup, experimental design, and analysis would be necessary.

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 ARCI 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.

Regarding the interest of our work for the GMD audience, it should be noted that we submit it to the inter-journal Special Issue *Chemistry–Climate Modelling Initiative*. Although the managing Editor should have agreed it is within the scope of the journal and the Special Issue, we would be open to move it from GMD to a counterpart journal.

Major Comments

1. I do not believe that with the current model namelist settings there is a realistic representation of aerosol-cloud-interactions (ACI). It is my understanding that WRF-Chem requires aqueous-phase chemistry combined with a modal/sectional aerosol scheme (MOSAIC or MADE/SORGAM) to model ACI. This experiment uses GOCART for aerosol, which is single moment in mass, whereas double moment in mass + number is required for ACI studies. I point the authors to the WRF-Chem User's Guide, which has a section on setting up the model for ACI.

I am familiar with the Thompson & Eidhammer (2014) aerosol-aware microphysics (MP), which backs out aerosol number information from the mass-only GOCART values via a lognormal aerosol distribution assumption. After digging around in the source code, I believe the module_mixactivate.F might do something similar for the Lin-GOCART setup. However, the specifics and whether or not and how the model is doing this transformation (or defaulting to a prescribed constant number when a sectional aerosol model isn't found) needs to be confirmed by the authors. This mass to number conversion does not make a scheme double moment because number is not a prognostic variable: it's inferred. This single moment approach is not enough to study ACI in a dynamical framework.

This is a well-argued concern. However, we confirm that although the microphysics implemented in the simulations rely on the Lin scheme, this single moment scheme turns into a double moment scheme in the simulations denoted as ACI. See details on how ARCI are implemented in the simulations in the response to the reviewer's comment #2 below. We have also added these details in the manuscript.

2. It's not considered ACI by the community to run a non-chemistry WRF simulation with a prescribed constant CCN number (single moment cloud) to a simulation with dynamic aerosol (double moment cloud). The change in moments and the change in CCN are intertwined and you cannot deconvolve these changes from each other. It is more realistic to run two WRF-Chem simulations with scale emissions and to run everything in double-moment.

As aforementioned, the Lin scheme is a single moment scheme based on Lin et al. (1983), including some modifications, such as saturation adjustment (Tao et al. 1989) and ice sedimentation, which is related to the sedimentation of small ice crystals (Mitchell et al. 2008). It includes six classes of hydrometeors: water vapour, cloud water, rain, cloud ice, snow, and graupel. This scheme was one of the first to parameterize snow, graupel, and mixed-phase processes (such as the Bergeron process and hail growth by riming), and it has been widely used in numerical weather studies.

The one-moment microphysical scheme is, effectively, unsuitable for assessing the aerosol-clouds interactions as it 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. Therefore, prediction of additional moments allows greater flexibility in representing size distributions and hence microphysical process rates.

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.

3. There is a difference in which autoconversion scheme is called between progn=0 and progn=1 in the Lin-MP (single moment vs double moment cloud). Some of the ACI attributed here is from the difference in the representation of autoconversion and not actually from ACI. This scheme change can be significant – see Liu et al. 2005 in GRL.

The reviewer is totally right. The autoconversion scheme activated with progn=1 (ARCI simulations), so that cloud droplets can turn into rain droplets, is different to the autoconversion scheme called with progn=0 (ARI simulations). Henceforth, this change in the flags of WRF-Chem configuration can lead to ARCI-ARI differences that cannot necessarily be attributed to aerosol-cloud interactions from a physical point of view, but also to different processes and schemes that play a role when progn flag is changed from 0 to 1. In this same sense, 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 (e.g. activation of aqueous chemistry or wet scavenging processes), that could also have an added impact to the effect that can be strictly attributed to the aerosol-cloud interactions. However, the encoding of WRF-Chem model imposes that ARI experiments should be performed with progn=0 in order not to allow an on-line calculation of cloud condensation nuclei, while ARCI experiments should be run with progn=1 if the on-line estimations of aerosols wants to be used not only for the radiative balance, but also for CCN (which change between progn=0 and progn=1 simulations). This is true not only with the Lin scheme used here, but also with the Morrison microphysics parametrization (the other scheme available including a double-moment mycrophysics). Therefore, ARCI-ARI differences can not be strictly 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 (that involves several modifications, including the autoconversion process as stated by the reviewer). All these unavoidable changes are intrinsic to the definition of the flags leading to the representation of aerosol-cloud interactions in WRF-Chem executions. We have now emphasized in the manuscript this aspect of the model configuration and its potential repercussion when interpreting the signals.

4. This manuscript makes no attempt to attribute the results to physical processes for ACI. Why are we seeing these results? What microphysical or environmental processes are actually causing the change in cloudiness? It's not enough to simply state that the change occurs. Most of the results section of the manuscript is describing what is on the plots and not interpreting the physics.

We now make more emphasis in attributing the signals to the direct, semi-direct and indirect aerosols effects.

5. The WRF simulations are compared to the coarse GCM for validation. Why not compare them (at least in the present-day scenario) to reanalysis that is run at higher resolution? There is no validation of the model against observations. At least reanalysis incorporates observations and is a start for validation.

The manuscript now includes a brief comparison of the present-day simulations with ERA5.

6. It is not clear what value is added by including gas-phase chemistry in these simulations. The pathways that contribute to aerosol are not explained.

Chemical reactions in the GOCART model include several oxidation processes by the three main oxidants in the troposphere: OH, NO3, and O3. The OH radical dominates oxidation during the daytime, but at night its concentration drops and NO3 becomes the primary oxidant (Archer-Nicholls et al., 2014). So, the oxidation pathways represented in GOCART include: (a) the dimethyl sulphide (DMS) oxidation by the hydroxyl radical (OH) during the day to form sulphur dioxide (SO_2) and methanesulfonic acid (MSA); (b) the oxidation by nitrate radicals (NO3) at night to form SO_2; and (c) the SO_2 oxidation by OH in air and by H_2O_2 and tropospheric ozone (O_3) in clouds (aqueous chemistry) to form sulphate (Chin et al., 2000). Henceforth, the skilful characterization of gas-phase radicals such as OH and NO3 or compounds like O_3 is essential for the representation of oxidation pathways in the atmosphere leading to the formation of secondary aerosols (Jiménez et al., 2003). Therefore, in this contribution the RACM (Stockwell et al., 1997; Geiger et al., 2003) mechanism has been coupled to GOCART trough the kinetics pre-processor (KPP) in WRF-Chem in order to provide the concentrations of radical and gas-phase pollutants needed by the GOCART aerosol model. We have added this explanation in the text.

References:

Archer-Nicholls, S., Lowe, D., Utembe, S., Allan, J., Zaveri, R. A., Fast, J. D., Hodnebrog, Ø., Denier van der Gon, H., McFiggans, G. (2014). Gaseous chemistry and aerosol mechanism developments for version 3.5.1 of the online regional model, WRF-Chem. *Geoscientific Model Development*, 7, 2557–2579.

Chin, M., Rood, R.B., Lin. S.-J., Müller, J.-F., Thompson, M. (2000). Atmospheric sulfur cycle simulated in the global model GOCART: Model description and global properties. *Journal of Geophysical Research*, 105(D20), 24671-24687.

Geiger, H., Barnes, I., Bejan, I., Benter, T., & Spittler, M. (2003). The tropospheric degradation of isoprene: an updated module for the regional atmospheric chemistry mechanism. *Atmospheric Environment*, 37, 1503 - 1519.

Jiménez, P., Baldasano, J.M., Dabdub, D. (2003). Comparison of photochemical mechanisms for air quality modeling. *Atmospheric Environment*, 37, 4179-4194.

Stockwell, W. R., Kirchner, F., Kuhn, M., & Seefeld, S. (1997). A new mechanism for regional atmospheric chemistry modeling. *Journal of Geophysical Research: Atmospheres*, 102, 25 847 - 25 879.

7. Breaking up the contribution to AOD and to ACI by aerosol type would be useful (e.g. carbon and dust will not have the same effect on CCN number as sulfates).

We can not afford to disentangle the contribution of each aerosol type to the effects attributed to the activation of the aerosol-clouds interactions in the simulations. It would require to run the ARCI simulations including only one of the aerosol species (5 in GOCART) at once. But each ARCI run takes months to be performed due to its expensive computational cost. So this is not a feasible task for us in a reasonably time.

On the other hand, we found that the main driver for the differences between the runs with aerosols and the runs without them is cloudiness, while the AOD plays a secondary role, which justifies the low attention paid to disentangling the contribution of each aerosol species to the AOD.

8. The overarching narrative of the paper is not clear. Is the point to compare RCM static aerosol to RCM dynamic aerosol? To assess the value added from moving from GCM dynamic aerosol to RCM dynamic aerosol? By the end of the paper, I had completely lost track of science question.

We have made an effort to make it clearer, starting by the title. The point is to evince the impact of aerosol-radiation and aerosol-cloud interactions in WRF runs performed with dynamic aerosols by comparison with baseline WRF runs performed without aerosols (nor dynamic, nor static). The baseline set-up is the most common one in the currently available portfolio of regional climate change scenarios provided under the umbrella of benchmark initiatives such as Euro-Cordex.

9. The English needs reviewing throughout the manuscript. More time is needed to revise the grammar and spellings than can be provided here.

Done with the help of a native speaker.

Specific Comments (page),[lines]

1. (2),[34-35] – What are GCMs modeling dynamically that RCMs are not?

It was said: “This is the case of the atmospheric aerosols concentration and their multiple non-linear interactions (eg. Taylor et al 2012 vs. Ruti et al 2016), the so-called aerosol-radiation and aerosol-cloud interactions (Boucher 2015).”

2. (4),[84] – Why WRF-3.6.1? It’s on version 4.2.1 now. Why such an old version?

Some of the simulations included in this work were performed time ago. Others have been performed more recently, but we decided to use the same WRF version to be sure of being comparing the same “thing”. At that time, when first simulations were carried out, the last stable version of WRF was the 3.6.1. In any case, the physics of the model is the same, no matter of its version.

3. (4),[87] – Why use GCM boundary conditions and not reanalysis? The CORDEX protocol suggests running the present-day experiments in the “perfect boundary condition experiment mode” with reanalysis and then running the future RCP scenarios with GCM boundary conditions.

We used GCM boundary conditions because we were to also asses impacts on future projections (not only sensitivity under present climate). Nonetheless, we have at our disposal a set of identical runs (BASE, ARI and ARCI configurations) using the reanalysis ERA20C and initial and boundary conditions. We are aware that the Euro-Cordex protocol establishes the use of Era-Interim as “perfect boundary conditions”, but we needed a longer period (for reasons that are irrelevant here)

and used ERA20C instead. The results from these simulations are attached below. These basically resemble those already included in the paper. Therefore, we decided not to include them in the paper for the sake of brevity.

4. (5),[98-99] – What is meant here by aerosol radiation is an external forcing?

This sentence was misleading and has been removed.

5. (5),[130-131] – The manuscript needs to stand on its own. If the focus of the paper is on ACI, then ACI in the model and model limitations in representing ACI within the setup and the resolution need to be described in full detail here.

We have extended the description of the model configuration and discussed about it, as explained above.

6. (5),[139-141] – See major comment #1

We now explain better this aspect of the model configuration.

7. (6),[144-146] – So the data was subset by the researchers for non-cloudy days? Radiation code often outputs clear-sky values. Why not use that to ensure a constant data stream?

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

8. (6),[144-146] – Is clear-sky only for that grid box where the threshold is met or is more data around those grid boxes removed?

The criteria is applied at the grid-box level, for each grid-box individually and independently. This has been also clarified in the text.

9. (6),[144-146] – Why do the clear sky values matter? Need to tell the readers why these are useful metrics to include.

We have included: “The analysis in absence of cloudiness will tell us about the relevance of the direct radiative effect of aerosols.”

10. (6),[154] – I’m lost in how averaging was done throughout this section and which time scales we are looking at. Are these a daily daytime mean that was then averaged into summertime means? Was the data filtered to exclude nighttime values?

We have better explain it in section 2 (in the new subsection 2.3 – Data and methods). We simply averaged over all the JJA (or either season) records in the series.

11. (6),[154] - The methodology for calculating the correlations, (especially temporal correlations) needs to be described.

Done in section 2 (in the new subsection 2.3 – Data and methods). The codes used are also made available.

12. (6),[156-158] – Wouldn’t the solar industry also be interested in effects under reduced solar output times (i.e. winter)?

We now included winter plots in Supp. Material and discussed about the seasonal dependence of our results.

13. (6),[156-158] – The direct radiative effect is strongest in summer, but what about the indirect effect?

Our results do support the key role of the indirect aerosols effect in summer indeed.

14. (7),[172-173] – Why is the spatial pattern in the response occurring? Why do some parts have an increase and some have a decrease? What is happening microphysically? Is it a difference in aerosol type that is causing this?

The increase in cloudiness in central and northern regions in ARI and ARCI simulations as compared to BASE could be explained through the following feedback mechanism: the cooling effect of the scattering of radiation by the high presence of sea salt, dust and sulphates over these areas cools down surface temperatures, thus increasing relative humidity and favouring the formation of clouds, which leads to less radiation reaching the surface, thus lower surface temperatures, and so on. Nonetheless, these signals would simply indicate that such a enhancing mechanism prevails over others, such as the semi-direct effect that acts to suppress cloudiness.

The reduction in cloudiness southward also appears in both ARI and ARCI simulations, but it is more evident in ARCI. Therefore, both semi-direct and indirect aerosol effects would tend to diminish cloudiness southward, with the latter (indirect effect) holding the strongest impact. This could be due to the fact that a high presence of large aerosols over southern Europe, both in form of dust or sulphate in our case, hampers the formation of clouds and enhances precipitation (shorter-lived clouds) as long as aerosol-cloud interactions are resolved by the model, which is a plausible explanation especially in the warm season over warm areas (e.g. Lee et al., 2008).

We have added and supported these arguments in the main manuscript.

References:

Lee, S. S., Donner, L. J., Phillips, V. T., & Ming, Y. (2008). Examination of aerosol effects on precipitation in deep convective clouds during the 1997 ARM summer experiment. *Quarterly Journal of the Royal Meteorological Society: A journal of the atmospheric sciences, applied meteorology and physical oceanography*, *134*(634), 1201-1220.

15. (7),[177-181] – The wording here is confusing. Differences of what exactly? Is the point to say that CTT reduces RSDS more than AOD? This needs more explanation.

Yes, that is the point. We have further developed this part to make it clearer.

16. (7),[184-185] – I don't see how the explanation in the previous paragraph proves this connection.

It is supported by the fact that differences between pairs of experiments in CCT correlates more than differences between pairs of experiments in AOD with the differences between pairs of experiments in RSDS. This showed up both, spatially (see s_corr values in Fig 1d-i) and temporally (Supp Fig 5a-f).

17. (8),[204-205] – How does the previous point imply orbital issues or water vapor? The link is not clear.

Different days (dates) may have different daytime lengths and different atmospheric compositions (thus different atmospheric optical depth or atmospheric transmissivity) that may mask the AOD effect under clear-sky conditions. We have better explained in the text what we meant.

18. (8),[206] – There is no transition into now looking at the future projections. Maybe split up into Section 3.A for present-day and 3.B for future.

Done.

19. (8),[219] – Where was this specified in Section 2?

It was said:

“Anthropogenic emissions coming from the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP; Lamarque et al 2010) were kept unchanged in the simulation periods (we considered the 2010 monthly values).”

We have now further emphasized and discussed this feature.

20. (9),[241-243] – 5% compared to what? GCM? No aerosol?

Compared to the BASE experiment (without aerosols). Now specified.

21. (9),[247-248] – Why are RSDS and cloudiness not linked? What are the physics here?

It was said the opposite: “Differences in RSDS between experiments are in overall good agreement with the differences found in cloudiness”

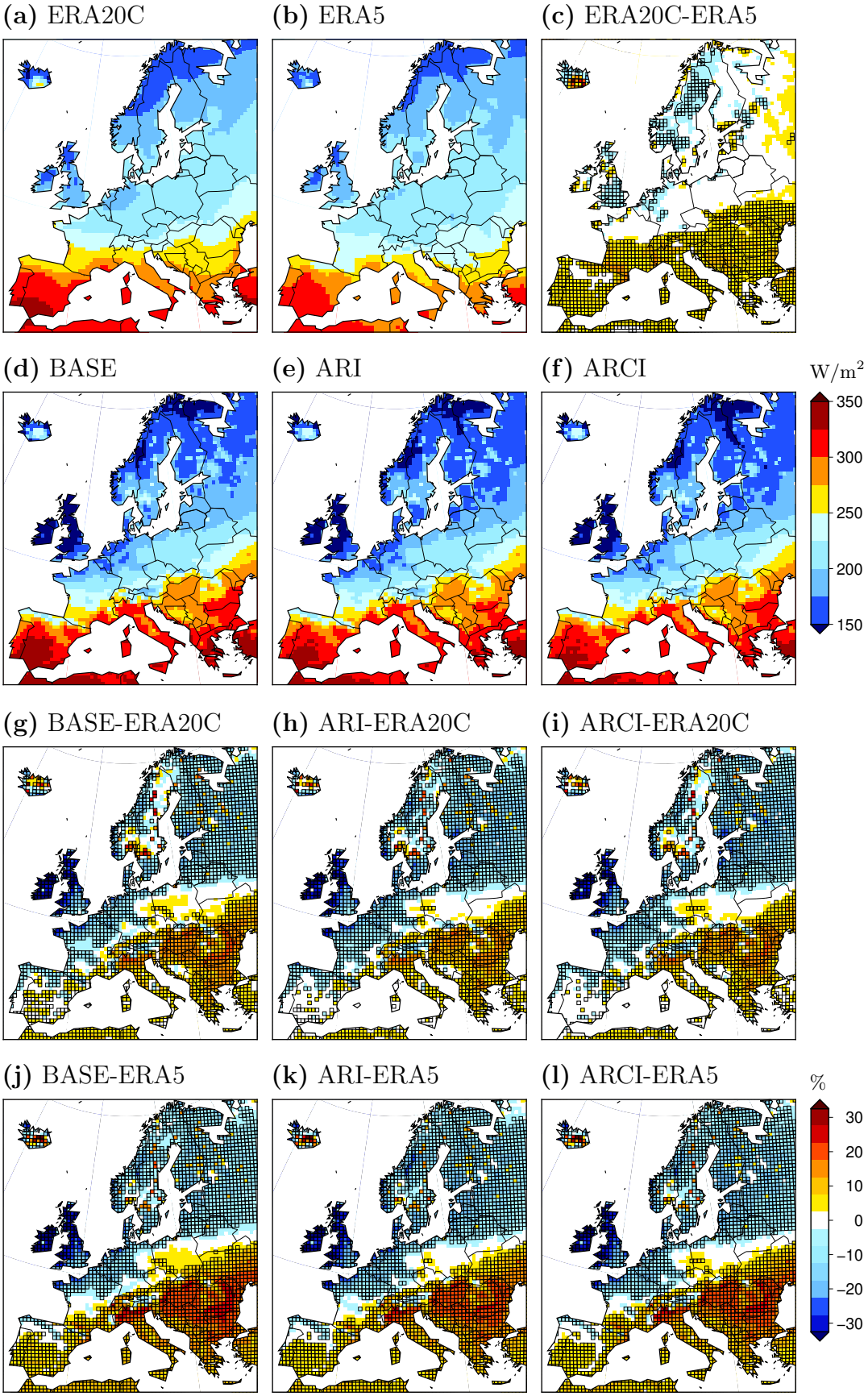
22. (9),[249-250] – What does this statement mean?

That statement was removed. It was certainly confused.

23. (9),[250-253] – Why is this conclusion significant in a broader context?

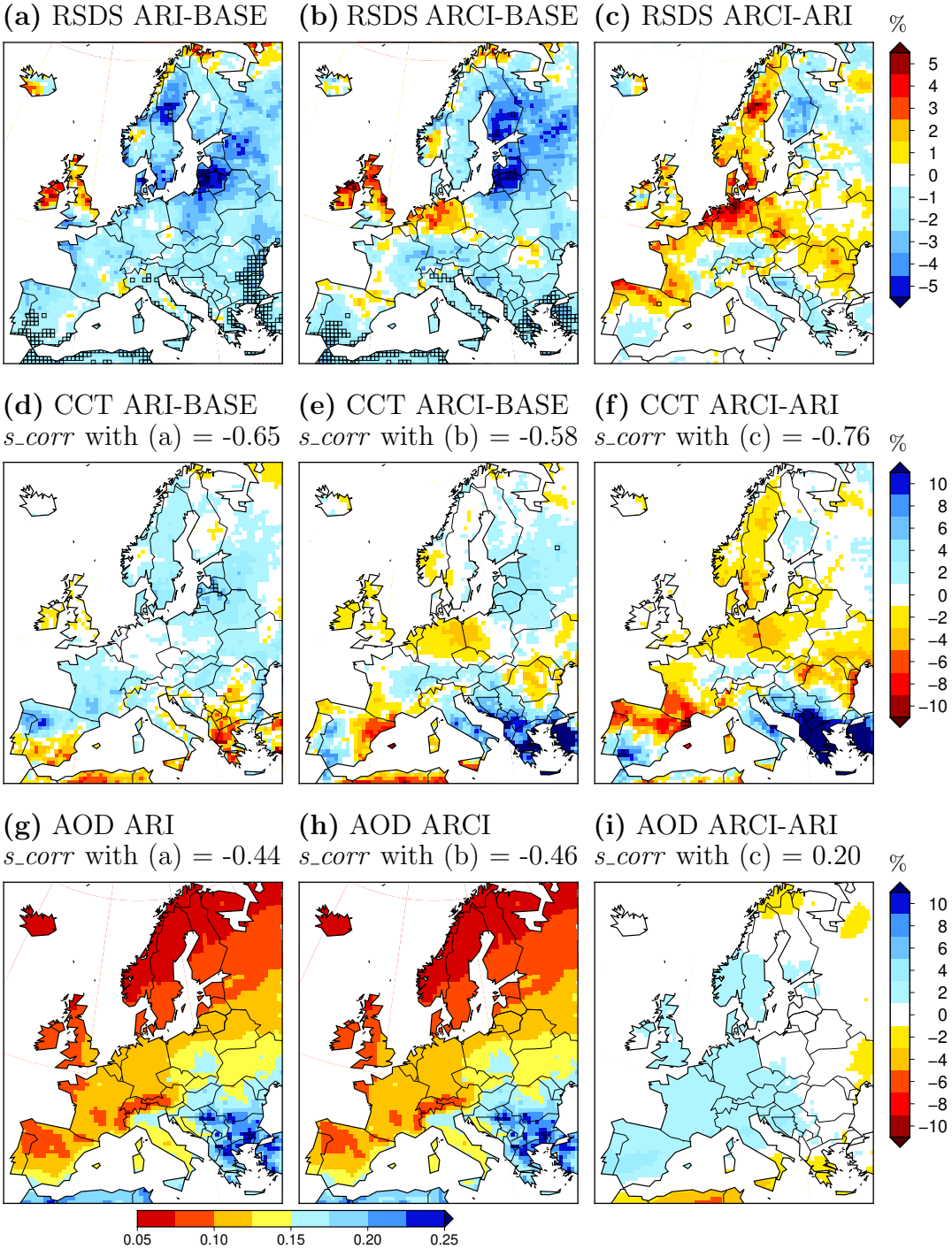
We now argument about the importance of the signals under clear-sky conditions.

RSDS JJA climatologies for 1991-2010



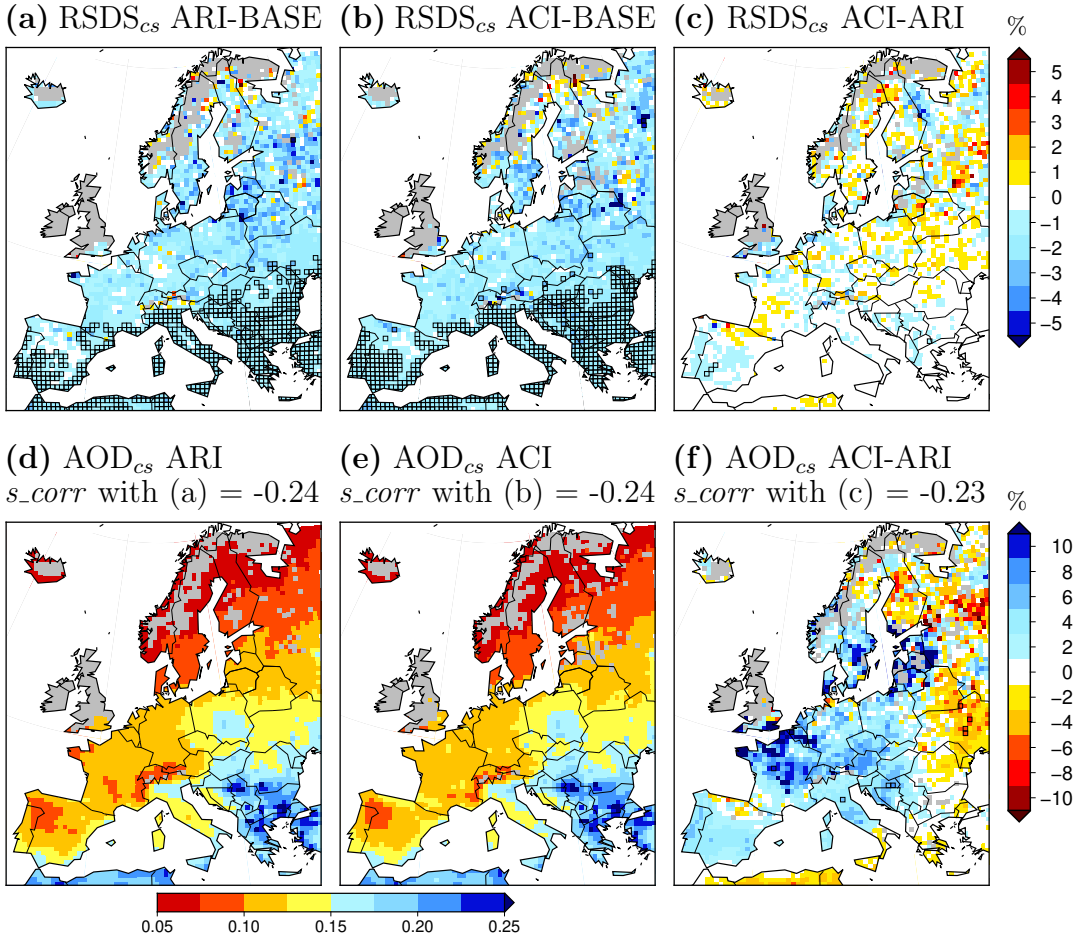
RSDS summer climatologies in the present period from ERA20C (a), ERA5 (b) and the ERA20C-driven WRF simulations (d to f); units: Wm^{-2}, same colorbar in all cases (the upper one). Panel c depicts relative differences between ERA20C and ERA5, panels g to i between each WRF simulation and ERA20C, and panels j to l between each WRF simulation and ERA5, squared if statistically significant ($p<0.05$); units: %, same colorbar in all cases (the bottom one).

RSDS, CCT & AOD JJA climatologies for 1991-2010: differences between experiments



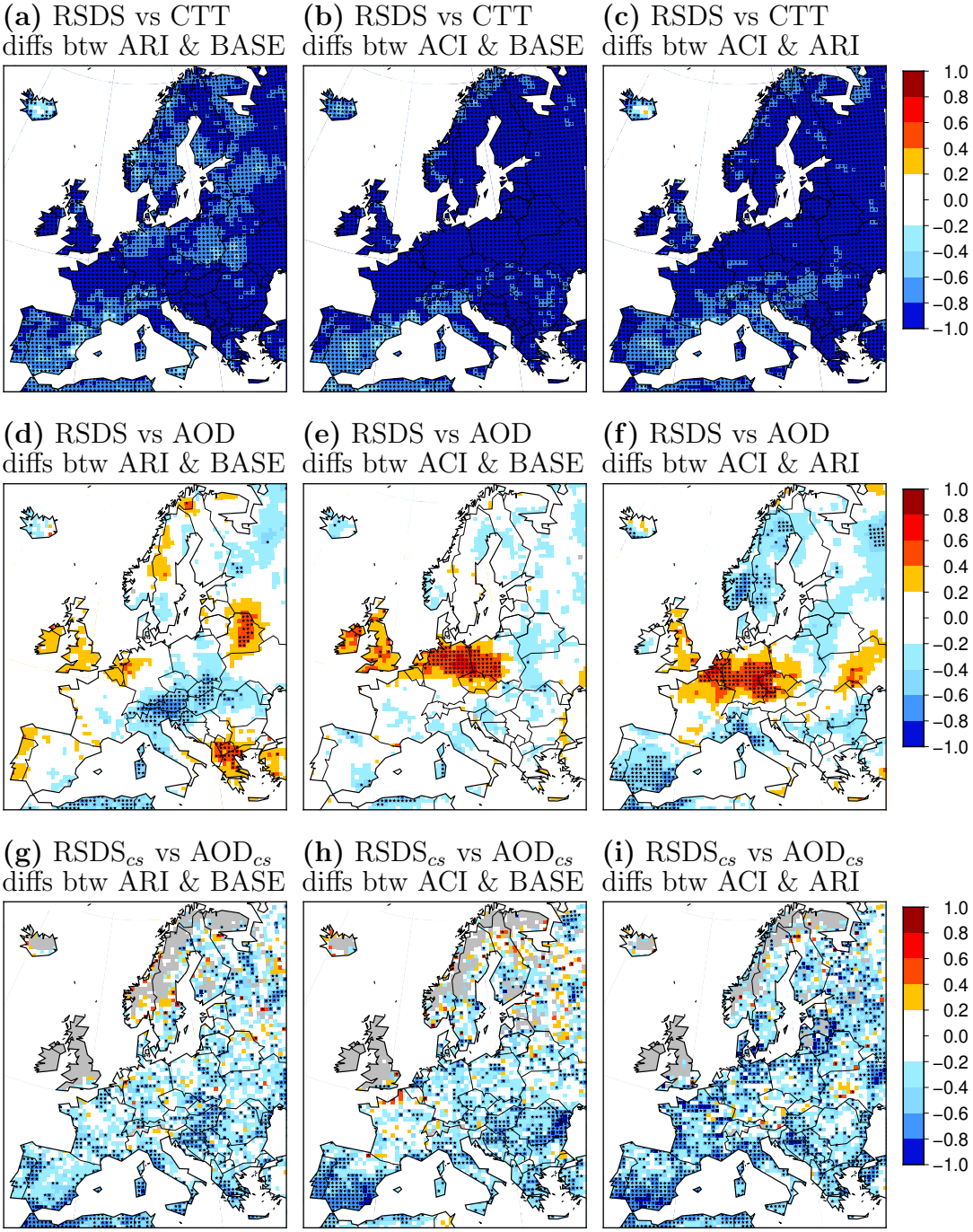
Relative differences between the ERA20C-driven WRF simulations in the RSDS (a to c), CCT (d to f) and AOD at 550 nm (g to i) summer (JJA) climatologies in the present period (1991-2010), squared if statistically significant ($p<0.05$); units: %. Note that panels g and h are referred to the horizontal colorbar just below them and simply represent the AOD summer climatologies in ARI and ARCI respectively. Spatial correlations (*s_corr*) between the patterns in the second and third rows and the respective patterns in the first row are indicated in the headers.

RSDS$_{cs}$ & AOD$_{cs}$ JJA climatologies for 1991-2010: differences between experiments



Relative differences between the ERA20C-driven WRF simulations in the RSDS$_{cs}$ (a to c) and AOD$_{cs}$ at 550 nm (d to f) summer (JJA) climatologies, this is under clear-sky conditions, in the present period (1991-2010), squared if statistically significant ($p<0.05$); units: %. Note that panels d and e are referred to the horizontal colorbar just below them and simply represent the AOD summer climatologies in ARI and ARCI respectively. Gray shaded areas depict grid point where less than 75% of the summer mean values in the time series of RSDS$_{cs}$ and AOD$_{cs}$ were not missing values. Spatial correlations (*s_corr*) between the patterns in the second row and the respective patterns in the first row are indicated in the headers.

Temporal correlations between difference series of RSDS, CCT & AOD: 1991-2010 JJA-mean series



As obtained from ERA20C-driven WRF experiments, temporal correlations between JJA-mean temporal series of differences in RSDS and CTT (a to c), RSDS and AOD (d to f), and RSDS$_{cs}$ and AOD$_{cs}$ (g to i; gray-shaded areas where the number of time steps in the clear-sky series is below 75% of total time steps) between ARI and BASE (first column), ARCI and BASE (second column), and ARCI and ARI experiments (third column) in the present period (1991-2010). Little stars indicate statistical significance ($p<0.05$).