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Geoscientific Model Development Discussions

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 #1

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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.

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.

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.

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.

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.

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.

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.

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).

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.

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

Specific Comments (page),[lines]

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

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

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

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.

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

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?

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?

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

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?

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

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

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

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?

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.

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

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

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.

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

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

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

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

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

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