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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Aerosol indirect effect on the grid-scale clouds in the two-way coupled WRF-CMAQ: model description, development, evaluation and regional analysis" *by* S. Yu et al.

Anonymous Referee #1

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This paper describes how the two-way coupling of the WRF and CMAQ models has been extended to include aerosol indirect effects. The authors describe the treatments taken from other models that are included in WRF-CMAQ. Then, they run the modeling system for one time period and evaluate the simulation, using primarily CERES measurements of top-of-atmosphere outgoing shortwave and longwave radiation. In general, the motivation for several aspects of the work is lacking and does not present results in the context of other models that have similar treatments. The organization in some sections is poor and it is difficult to follow the text and understand the points the authors are trying to communicate. There is one instance of papers being cited having

little to do with statements made in the text. I have not checked whether there are other such instances. It is evident that a lot of work went into this study; however, this paper reads like a first draft for the reasons listed below.

Some of my major comments summarize one or more of the more important specific comments. I made fewer specific comments towards the end the manuscript because of the major concerns, but the authors should assume there are not any problems in those sections.

Major comments:

1) There is a lack of motivation for several aspects in this paper. First, the results in Section 4 imply that including aerosol effects are important (which has been shown before in other studies), but the initial motivation for the study is lacking. The motivation material presented in the introduction is a summary of the importance of the aerosol indirect effect from a climate perspective, without any transition into the justification for the present work. The modeling system is a regional one that will rely on global models for boundary conditions. So is the modeling system designed as a downscaling tool, to study climate relevant processes, or include climate relevant processes for air quality applications? Second, since the authors are using CMAQ, an air-quality model, I assume that WRF-CMAQ will be used for air quality applications. However, there is no motivation as to why simulating aerosol indirect effects are significant for air-quality applications. A few concise statements on the purpose of the modeling system are needed. Third, there is no motivation for the domain or time period chosen. I recommend a separate section for that discussion. Forth, there is also little motivation for the observations used to evaluate the model performance. Finally, there is no motivation why two radiation schemes are compared when there could be many types of sensitivity studies with different parameterizations that would affect the aerosol indirect effect in some way.

2) In Section 4, the results for the simulated air quality metrics are presented first before

investigating how aerosols affect a select set of cloud properties and radiation. On one hand, it may make more sense to first evaluate if the model is simulating the aerosol indirect effect correctly before presenting impacts on air quality metrics because this is the first application of the model. On the other hand, the aerosol indirect effects will depend on the simulated aerosols. If the authors choose to leave the order the same in this section, a better transition is needed. Results from simulations with and without the aerosol indirect effect are shown for Section 4.2 on cloud properties, but only simulations with aerosol indirect effects are shown for Section 4.1 on air quality metrics. For consistency, results of simulations without the aerosol indirect effect should be included in Section 4.1 so that the reader can determine whether the inclusion of those effects has any impact on air quality metrics. Another point to mention is that the evaluation of surface aerosol concentration may or may not bear any relation to aerosol indirect effects since they are not actually within the clouds. I'm not expecting an evaluation of aerosol concentration aloft (it would be advantageous though), but it is hard to tell how errors in simulated aerosols will affect the effects on clouds and radiation shown later.

3) The study shows that including aerosol indirect effects improves some metrics for cloud properties, but this has been shown before in other studies. For example Saide et al. (2012) and Yang et al. (2011) used WRF-Chem (with very similar treatment of aerosol indirect effects as in this study) to show that cloud effective radius (Twomey effect) is improved when full aerosol chemistry and aerosol indirect effects are included. This is just one example, and this study ignores many other modeling studies on regional-scale simulations of the aerosol indirect effect that could be used to put their results into the context of other models. It would be useful to compare their results with other studies for consistency wherever possible, especially since this is the first application of the modeling system.

4) In the introduction, three approaches of coupling between meteorology and aerosols in models are discussed. The authors mention WRF-Chem as an example where aerosol chemistry and indirect effects are added to an existing meteorological model,

and then mention that another approach (GATOR-CGMOM) is to have that coupling enabled when the model is first created. The sequence of model development is less important than the technical methodologies used to enable the aerosol-cloud interactions. Both of these approaches fully integrate meteorology and atmospheric chemistry into one model and I do not see much difference between the two. The authors do use a different approach for WRF-CMAQ with a more "loose" coupling of two models; however, they do employ many of the same approaches of handing aerosol-radiation-cloud interactions as in WRF-Chem. But the authors do not mention that. They seem to imply (page 25654, line 19) that there are few studies on regional scale coupling of meteorology and aerosols. There have been numerous studies and the number of such studies has increased dramatically the past three years. For these reasons, I find much of the text in this paragraph misleading in that it implies that WRF-CMAQ is the only regional modeling system that includes the indirect effects.

5) Since the authors employ the treatments of aerosol indirect effects on liquid clouds that have been available in the publically available WRF-Chem model for several years, it is also not clear how similar or different the author's contributions to WRF-CMAQ are to WRF-Chem, which could be very confusing for potential user. The authors even use the term "CMAQ_mixactivate" on page 25657, line 18. There was a subroutine already in WRF-Chem called "mixactivate" to handle the cloud droplet activation by aerosols using the Abdul-Razzak and Ghan scheme. Did the authors modify and/or copy that subroutine to handle CMAQ aerosols? If so, citing that previous work on their performance is warranted. It would be useful to have a short paragraph in the model description section to compare and contrast the methodology used in the two codes.

6) The treatment of homogeneous and heterogeneous ice nucleation in models is highly uncertain, but I could not find any mention of this in the in the text. The ice number will have large effect on radiation. There is no evaluation of cloud ice particle concentration or IN concentration, only an indirect evaluation of the effects of aerosols on cloud radiative properties. One of the co-authors on the paper is an expert on IN

parameterizations and I am bit surprised that some text has not been included to note the potential uncertainties in the treatment aerosol effects on ice phase microphysics. While I do think that the inclusion of aerosol effects does improve the simulated cloud properties, similar differences in magnitude could also be obtained by running WRF with other parameterization choices.

7) Is advection, vertical mixing, and diffusion in CMAQ and WRF different? I assume that they are so, since CMAQ is intended to be used for both off-line and loosely-coupled applications. It seems that CMAQ can been called less frequently than the time step in WRF. It is not clear how those different time steps affect processes influenced by clouds. Clouds can change rapidly and the aerosol indirect effects would be simulated at the meteorological time steps for on-line models. For the approach in this study, there is a bit of a disconnect between the evolution of clouds and aerosols. Have the authors done a study with CMAQ simulated at the same time step as the WRF to show that those effects are minor? The magnitude of those differences may depend on the spatial scale used by the model as well. Ovtchinnikov and Easter (2009) show that advection errors can significantly impact aerosol-cloud interactions. Some discussion is warranted somewhere in Section 2 regarding the numerical aspects of how their coupling could lead to different aerosol indirect effects than fully online calculations.

Specific Comments:

Page 25652, line 17: Delete “so-called”. I do not know what the authors are trying to imply with this phrase.

Page 25653, line 13: Change “medium-low” to “medium to low”.

Page 25653, Lines 16-17: The authors are expecting the reader to know that these magnitudes (which are global averages) are large. Some perspective is needed here for those not familiar with global climate model metrics.

Page 25654, lines 7-12: Satellite measurements are not the only means of evaluating

model simulations in terms of the effect of aerosols on clouds and the effects of clouds on aerosols.

Page 25654, line 15: The use of the term “air-quality” is one from the EPA perspective in which a chemistry model has been developed from the perspective of computing concentrations of trace gases and particulates related to human health. However, a climate model needs some sort of treatment of aerosol chemistry that is coupled with meteorology and includes treatment of aerosol-radiation-cloud interactions. Numerous atmospheric chemistry models (not necessarily used for air-quality applications) have been used with global and regional meteorological models to simulate climate and climate-relevant processes. So I do not think the use of air-quality here is entirely correct.

Page 25655, line 10: Please indicate where the model is available.

Page 25665, lines 15 -29: Much of the discussion here on how aerosol indirect effects are included in the model seems to be too detailed for the discussion session. This just seems to be reiterating what was presented in previous studies, and it would be more appropriate and sufficient to just describe the specific processes themselves that were included. So perhaps this section could be cut back a bit.

Page 25656, line 2: Perhaps some references are needed after “many studies”. Also, it is not really a good justification as to why they are using these two schemes. A better one is that they are the latest, and presumably better, schemes used by a select number of models. Not all models use these schemes so it will not be apparent to many readers why these are used.

Page 25656, lines 3-5: Here and later in the text, there is absolutely no rationale as to why this particular domain and time periods is chosen. Since this is a paper on aerosol indirect effects, what makes this period and domain useful to study those effects? Since this is a first application of WRF-CMAQ in this manner, it is not clear why this is the best case to evaluate the model’s ability to simulate aerosol indirect effects that seem plausible.

Page 25658, line 19: Starting here and continuing on the next page are abbreviations of various compounds. These all seem to be CMAQ specific acronyms and as such are only meaningful to users of CMAQ and I do not think they are needed.

Page 25659: lines 18-19: This sentence seems to be a random thought that is not relevant to the rest of the paragraph. More disturbing is that five other papers from the first author are listed to cite work regarding the tradeoffs of accuracy and computational expense between the modal and sectional approaches; however, the first four papers do not even mention this topic and the Yu et al. (2008) papers only provides a similar sentence (on page 3 of that paper) "Generally speaking, the modal approach offers the advantage of being computationally efficient, whereas the sectional representation provides more accuracy at the expense of computational cost." However Yu et al. (2008) is not a paper comparing the computational expense between the two approaches. Interestingly, this sentence is nearly quoted nearly verbatim in the earlier McKeen et al. (2007) paper on page 3: "As a general rule, the modal approach offers the advantage of being computationally efficient, whereas the sectional representation provides more accuracy at the expense of computational cost." In the next sentence by McKeen et al. (2007), there is a correct citation on comparing modal and sectional approaches. Liu et al. (2011) also does not even mention this topic. So it seems that the authors are providing no citations here that are relevant to their statement, yet there are many such papers on the topic.

Page 25658, lines 19-21: There is only one sentence on the chemical boundary conditions. A little more description is needed. Do the boundary conditions vary in time? Are the aerosol species in GEOS-Chem the same as in CMAQ? If not, how are the aerosols from the GEOS-Chem mapped to CMAQ? What is meant by the "annual 2006 GEOS-Chem simulation"? 2006 is mentioned here, but the case study period has not even been described yet.

Section 2.2: The authors summarize aerosol-cloud-radiation interactions that have been largely described previously, so I am not sure the level of detail is necessary. Are any of the details for how the authors implement aerosol-cloud-radiation interactions any different than the previously cited studies?

Page 25662, line 20-24: Some references are needed here and in Table 3 on how the hygroscopic parameters are chosen. Since they employ an approach similar to WRF-Chem, it would be useful for user's to know if they are different (or from other models for that matter).

Page 25663, lines 9-23: The discussion on interstitial and cloud-borne aerosols seems very similar to the methodology employed in WRF-Chem. There is not much discussion on how aerosol mass is moved between cloud-borne and interstitial aerosols.

Page 25664, line 1: Again, the publically available WRF-Chem code has already coupled aerosol chemistry to the Morrison microphysics scheme by including aerosol-cloud interactions (Yang et al. 2011). Is the present approach different or the same? If it is the same, a citation is important. If it is not, then the differences should be articulated.

Section 2.2.2: What is missing from this section is a discussion of the uncertainties in IN parameterizations. Parameterizations, like the one used in this study, have been compared with field observations to show that there is no best parameterization.

Page 25670, line 14: The phrase "both models" is not quite right. The authors are using one model with two different parameterizations.

Page 25671, line 8: I think there are plenty of acronyms used in the paper, and "WUS" and other uses like this here and elsewhere is not needed.

Page 25671, line 20: It is not clear the averaging period for the numbers quoted here. Are the factors based on 1-h average or 24-h average?

Page 25672, lines 6-7: It is not clear how this statement follows the previous material that says SO4, NO3, and NH4 are too high. Is it because too few clouds means too little wet removal? Be specific and clarify.

Page 25674, line 16: The authors here mention the performance for cloud fields. There are several places in the text where this is done. It may be more clear if the meteorological performance is discussed prior to the discussion on PM2.5.

Page 25675, lines 5-19: The discussion is missing something about how the model results are averaged in time to match the CERES observations where monthly averages were created (as stated in section 3.2). Since the CERES results are at a coarser resolution (1x1 degree) than the model, it would make more sense to average the model results to the 1x1 degree grid for statistical and graphical comparisons.

Page 25675, line 21: The paragraph starting on this line seems to have no connection to the following material. The paragraph talks about the ratio SWCF/LWCF and that concept does not seem to be brought up until much later for Figs. 17-18. Why not move that material closer to the discussion of the results?

Page 25676, line 26-27: I think it is highly unlikely that the reason for the underestimation of cloud could be due to the lack of aerosol indirect effects in subgrid convective clouds.

Page 25677, line 1: This line refers to figures 11 and 15. It seems that there was no text describing Figures 9 and 10. If figures are not described, why are they included in the paper? This is very confusing. It is also confusing as to why the reader has to jump from Figure 11 to Figure 15. If they are being talked about in the same context, maybe the figures need to be together. My same comments hold for all the description on the tables and figures in Section 4.2. The text seems to skip figures and jump around quite a bit and it is very hard to follow the text throughout this section.

Page 25677, lines 2-3: This is a strange sentence. The 4 km simulation resolves clouds that the 12 km simulation cannot, and thus are “subgrid” in relation to the 12

km simulation. But the way the sentence says the 4 km simulations resolves subgrid clouds, which is not entirely true. A large fraction of convective cells will be at smaller scales that 4 km. The statement also says that both the higher resolution and inclusion of aerosol captured the SWCF and LWCF better than the simulation without aerosols. This is true from the domain averaged statistics in Table 9, but it is not that clear from Figures 10 and 11.

Page 25678, lines 16-18: There are likely many cloud dynamists who would disagree with this statement. And it depends what metric one is looking at. While top of atmosphere radiation may be improved, what about other factors from the models that are more relevant to applications, i.e. near-surface temperature and humidity, precipitation, storm occurrence/frequency, etc. The authors do show metrics on surface aerosol concentrations and precipitation. While aerosol concentrations are improved, precipitation is less convincing. One could simply change the turbulence, convection, and/or microphysics parameterizations and obtain similar or bigger differences.

Page 25682, Section 5: Most of this text is summary material so that the section title should probably be changed to “Summary”. The conclusions do not appear until the last short paragraph.

Figure 1: The figure caption does not say whether the results are a monthly average or a snapshot in August. Given that the model domains are shown in subsequent plots, I’m not sure how much it adds here. Perhaps if it include the locations of surface measurements that were used to evaluate the model, for example show the simulated PM2.5 distribution compared to observations which is not done elsewhere.

Figure 3: The flow chart has no arrows from the droplet or ice effective radius to radiation, so it looks like there is no effect of aerosol-cloud interactions on radiation.

Figure 4: The caption should include the time period of the data points used. In the text sometimes the authors refer to August or September, so it is not clear if these results are one month or both.

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