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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessment of multi-decadal WRF-CMAQ simulations for understanding direct aerosol effects on radiation "brightening" in the United States" *by* C.-M. Gan et al.

Anonymous Referee #2

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This paper investigates the changes of anthropogenic emissions of SO2 and NOx in the United States over 1995-2010 and the subsequent impacts on the changes of SW radiation in all-sky and clear-sky conditions. The CMAQ-WRF model is used to simulate the changes of aerosol concentrations, AOD, and SW radiation, and the results are compared with observations from surface observation networks. It is found that with the decrease of the anthropogenic emissions of SO2 and NOx input to the model, the model is able to simulate the observed decreasing trends of SO2, sulfate, nitrate, and EC surface concentrations over the US, and to some degree, the AOD trends as well.

The model also reproduces the all-sky trends of SW radiation change, although it has difficulties in matching the clear-sky SW radiation trends.

The topic of the paper is well suited for ACP. However, I feel this paper does not deliver what the title says, that is, to use the WRF-CMAQ simulation for understanding direct aerosol effects on radiation brightening in the United States. Some important aerosol species (dust, organic aerosols) seem to be excluded in the model, the calculated AOD is much too low, only half of observed values in both west and east US, and the attribution of SW radiation change to aerosol change is not convincing. I recommend a major revision. My comments are given below.

General comments:

1. Aerosol simulations: It has not been made clear how many aerosol species are included in the model. The concentration figures showed sulfate, nitrate, and EC, but no indication on what are included in AOD and SW radiation calculations. I noticed the statement in Figure 2 and Figure 3 captions: "Note that for emission dataset, only SO2 and NOx are available". Does it mean that other aerosols and precursors are not available? If the model omitted other important species, like dust and organic aerosol, then the AOD and SW radiation calculation would be incorrect, and the paper would not be appropriate for assessing the aerosol effects on surface radiation trends.

2. Clear-sky and all-sky SW surface radiation trends: The all-sky brightening trends over both west and east US are evident from the SURFRAD stations, which are pretty much reproduced by the model. Even though there is little change of aerosol concentrations over the west US in the 16-year time period, both model and data show a clear brightening trend in the west under all-sky condition. On the other hand, the model is much less successful in reproducing the SURFRAD trends under clear-sky conditions when aerosol is supposed to be the driver for the clear-sky trends. Such results may suggest (1) aerosol is not the major factor responsible for the all-sky brightening trends, and (2) the change of anthropogenic aerosol simulated by the model cannot explain the

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observed clear-sky brightening trend. These issues are the core for this investigation and should be seriously addressed.

3. Multi-decadal: The analysis and comparisons in this work is from 1995 to 2010, i.e., 16 years, which is not “multi-decadal”. The title should be modified.

4. Uncertainty and error estimation and range of data: Because of the omission of the aerosol species in the model, it is necessary to provide uncertainties and estimated errors in model calculations. The uncertainty of the data should also be presented. In addition, the comparisons are shown for the aggregation of sites over multiple sites, the range or standard deviation of both data and model should be shown in the comparison figures, as well as the statistic measures (e.g., correlation coefficients, biases, errors, etc.).

Specific comments:

Page 17713, line 2-3: Does this opening sentence suggest you only consider sulfate and nitrate?

Page 17713, line 19-21: I don’t understand why the “irregular” aerosol distribution of aerosol would be a big challenge for measurements to quantify the aerosol forcing. To me, the big challenge is the clouds that may dominate the all-sky radiative flux and may still contaminate the “clear-sky” data. Besides, “heterogeneous” is a better word than “irregular”.

Page 17716, line 5: What aerosol and precursor species are included in this “comprehensive emission data”? This sentence contradicts the sentence in the captions for Figure 2 and Figure 3 saying “for emission dataset, only SO2 and NOx are available”, as I mentioned earlier. Does the wild fire emission included? And dust?

Page 17717, line 15: Add “that” in between “estimated trend” and “is statistically”.

Page 17717, line 21-23, sentence begins with “The size of the circle”: This is a bit out of context – are you talking about the circles in some of the figures? You should talk

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about this together with the figures.

Page 17718, line 4: “feedback simulations” – if the meteorological fields are “nudged”, how would feedback change them? Does feedback matter?

Page 17718, line 5-6: “. . .the observed and modeled surface aerosol concentrations. . . are presented in Figs 2 and 3” – SO2, shown in Fig. 2, is not aerosol. It is a gas.

Page 17718, line 20-22: “. . .the model predictions agree well with surface measured aerosol concentration” – there is no comparisons of actual concentrations, only the concentration anomalies are compared.

Page 17718, line 21 and 23, page 17719, line 2 and 12, and page 17723, line 23: “. . .well. . .” – how well is “well”? This is a subjective term and should be avoided. The degree of agreement should be presented by statistics, such as correlation coefficient, bias, and error.

Page 17719, line 8-9, trends of PM2.5, AOD: They don’t necessarily change together in the same directions, as several previous studies showed positive correlation in the eastern US for some seasons but weak or even negative correlations over the western US. The relationship depends on aerosol vertical distributions, compositions, among others.

Page 17719, line 22-23: The observed trend is 9 times stronger than the model simulated trend. Such difference means that the model is not able to “capture trends similar to observations”.

Page 17719, line 24-27: Which one of these possibilities is the most important one? What fire emission data are you using? Since dust is an important aerosol component in the west, omission of dust would be a significant problem for model. Sea salt contributions should be negligible for the sites shown in Figure 1.

Page 17720, line 1-4: Yes both model and data show a brightening trend, but is this trend due to the change of aerosols?

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Page 17720, line 13-15: Is the PM concentration underestimated in the model? By how much? The AOD is underestimated by a factor of 2.

Page 17720, line 25 to page 17721, line 2: Do you expect the anthropogenic emission should change with El Nino? How? Also, from Figure 6, the disagreement of modeled and observed AOD for 1998-1999 is similar to that for other years!

Page 17721, line 10-14: Indeed, if the clear-sky radiation change is dominated by aerosols, with the decrease of aerosols, the total and direct radiation should increase but the diffuse radiation should decrease. How is "clear-sky" defined in the SURFRAD data? What is the uncertainty in the clear-sky radiation observations?

Page 17722, line 3-4: The model-data discrepancy is much larger for clear-sky than for all-sky. If the implementing aerosol indirect effect might help improve the all-sky simulation, it would not be helpful for clear-sky simulations.

Page 17721-17723 of Section 3.2: The comparisons and discussions of radiation trends should be better organized. It is said in line 3-4 that " this study focuses on clear-sky SW radiation in the following discussion", but then the all-sky and clear-sky results are shown alternately without much organization.

Page 17723, line 25-27: There is no sufficient evidence to point that the inaccurate emission before 2000 is responsible for the model-data discrepancy of AOD and radiative fluxes. As I said earlier, the agreement of simulated AOD and concentration trend before 2000 are similar to that after 2000.

Page 17723, line 2-4: If the model cannot reproduce the trends over the available observation sites, will it be a mistake to use such model to fill the observational gaps?

Page 17723, line 12: "...influence by local terrain influences" – how? What are the references? This is not mentioned anywhere in the previous sections.

Table 3 and 4: What quantity is in the "significance" column? Needs clarification.

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