

Interactive
Comment

Interactive comment on “Climate impact on airborne particulate matter concentrations in California using seven year analysis periods” by A. Mahmud et al.

Anonymous Referee #1

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This paper discusses results from simulations with a model formed by linking other models used on different scales together to estimate the effects of future emissions from multiple sources simultaneously on California air quality. At the highest resolution (8 km), a chemical transport model (CTM) is used for the simulations. The study concludes that, when a long time period (7 years in this case), predictions of PM_{2.5} are not statistically significant over most of California.

Overall, I believe the authors are headed in the right direction and make an important point about the need for simulating over a long period and at high resolution. I also enjoyed reading this article.

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At the same time, however, I do not believe the authors can draw the conclusions they have so firmly from the simulations they have run. Further, I see no evaluation of the meteorological (weather or climate) component of this model even though this is the first application of this combined set of models to date that I am aware of. As the paper focuses on climate impacts, it is essential that the model be evaluated for climate parameters. For example, precipitation and temperature data are readily available in California. At a minimum, the authors should show that their model can predict these parameters climatologically over California.

Once this is done, if the authors can be extremely clear about the shortcomings in the simulations that I will mention below and modify the abstract, text, and conclusions accordingly, I would support publication of the manuscript since I believe the main point the authors are making, that high resolution and long simulations are needed, is an important one.

Shortcomings that need to be clarified:

1. The model does not treat feedbacks of aerosols or gases to meteorology or clouds at the highest resolution since a CTM is used at that resolution. As such, there is no treatment of the indirect (microphysical) effects or radiative effects of aerosols on clouds, which are known to be important. There are also many other feedbacks not treated. The authors should state specifically in the abstract and conclusion and text that their model does not include feedbacks at the highest resolution and their conclusion could change if feedbacks were treated.

2. The statistical significance tests used are not tests that are typically run to determine the significance of perturbation on climate. Generally, statistical significance of a perturbation in a climate model is determined by running a series of climate simulations for at least a year straight with a small perturbation in the initial conditions, then statistically comparing a sensitivity of interest to the results from the random perturbation tests. In the present case, the authors are running many short simulations for part of

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the year), and none include climate feedbacks at the highest-resolution. Because the simulations are all short (17 days) thus influenced more by startup variations (despite removing 4 days) than a single long simulation, and do not include feedbacks at the high resolution, it is not clear whether the authors can conclude that statistical significance of results is really determined. At a minimum, the authors should caution that because their simulations do not include feedbacks and are a series of short simulations, results (particularly of statistical significance) could differ if a single set of long simulations with feedbacks were run.

3. Figure 2a. indicates that the coarse-resolution model predicted higher ocean warming than land warming due to future emissions. This result is highly suspect and not evidenced by the historic record for global warming to date, so it is unclear why this result should be believed for the future. If the authors wish not to rerun all their simulations with a new global model result, they really should caution that their result could change significantly if a different temperature effect were found.

4. The authors imply in the title and abstract that 7 years were simulated. However, this is not the case. The authors picked intervals within seven years and ran several short simulations of 17 days (with 4 days removed) followed by 25 days of no simulation. I calculate that only 2.16 years of results were used (13 simulation days per 42-day period over 7 years). The authors need to be clear in the abstract and title that a set of short simulations < 7 years rather than a long simulation of 7 years was run, as running 7 years could give a different result.

5. The authors state in the abstract, “The present study employs the highest spatial resolution (8 km) and the longest analysis windows (7 years) of any climate-air quality analysis conducted for California to date.” However, the authors should point out that the vertical resolution used here (10 layers in the finest domain) is lower than that in several studies. Also, Jacobson (Environ. Sci. Technol., doi:10.1021/es903018m, 2010) examined global-urban nested results, with feedback, at higher spatial resolution, but shorter time, over Los Angeles and also for 2 years over California at lower

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horizontal but higher vertical resolution (compared with 2.2 years spread over 7 years here over California at higher horizontal but lower vertical resolution). The vertical resolution is stated in that study to include 35 layers in the Los Angeles and California domains. The authors should clarify their statement in light of this other work and clarify the difference in methodology (e.g., including feedbacks versus not including feedbacks; simulation of the effects of one chemical locally emitted in that study versus multiple chemicals simultaneously in the present study) so that results of the different studies can be interpreted in light of their assumptions.

Additional comments.

P. 2989. In the discussion of the authors' study, it is essential to discuss what major processes their model was including and missing. Was the model treating gas chemistry, effects of aerosols on photolysis, etc. What major processes were missing?

P. 2988. The authors state that it is important to simulate the ENSO cycle. However, I could not find evidence that not including the cycle would make a difference to the general conclusions determined from this study (or any other study). It may or may not be the case, but I think the statement that "air quality analysis must be carried out over a similar time scale in order to be climatologically relevant" is only stated here and not proven. I think the authors can safely say that longer simulations are more desirable since they are more likely to capture interannual variability and variability arising from cycles such as ENSO. If the authors want to make a firm statement, the authors should present some evidence that the model they are using can predict some elements of ENSO and that not including ENSO variability gives a different result.

P. 2990. The PCM used here has only 18 layers up to 4 hPa. This is very coarse resolution; about 1/3rd that used in more typical global models. The authors should acknowledge this coarse resolution as a major source of uncertainty. Further, it appears that the PCM does not predict the dynamically and chemically-changing ozone layer (e.g., it does not solve photochemistry). The authors need to specify that their model

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does not predict feedbacks to the ozone layer, and this is another potential source of uncertainty.

P. 2991, 2994. The authors should discuss how gas and aerosol wet removal is treated in all models they use. Is the treatment empirically-based or physically-based? Does the removal rate vary for particles with particles size and composition?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 2985, 2010.

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