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Atmospheric Chemistry and Physics Discussions

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

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Comment 1: "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."

Response 1. The evaluation of the meteorological fields produced by the downscaling technique was performed by Zhao et al. (2010a,b). These manuscripts have been

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recently submitted to the Journal of Climate for publication and so the details were not repeated in the present study. As a summary, the analysis in these papers shows that downscaled 10m windspeed was over-predicted during the January, February, March, October, and December episodes by 0.5-1.5 m sec-1 in the SoCAB and by 0.25-0.5 m sec-1 in the SJV. Generally speaking, WRF over-predicted windspeed during events with observed 10m windspeed less than 4 m sec-1 and under-predicted windspeed during events with higher windspeed. We believe this is a shortcoming in the WRF model itself (and possibly MM5) that will be common to all downscaling exercises in California. The PCM-WRF modeling system generally captures the pbl height during the fall, winter, and spring months when PM concentrations are greatest but over-predicts the planetary boundary layer height by a factor of almost two during the summer months. The accuracy of the PCM-WRF surface temperature predictions also vary with season. Temperatures at 2m during March, April, May were biased low by 1-2oC in both the SoCAB and the SJV. September 2m temperature was biased high by 6oC in the SoCAB and 5oC in the SJV, but further analysis shows that the majority of this temperature over-prediction was inherited from the PCM model. Likewise, July and October episodes over-predicted 2m temperature by 1-2oC but performance usually increased substantially when reanalysis products were used for boundary conditions rather than PCM output.

The general accuracy of the meteorological simulations are inherently captured by the comparison between predicted vs. measured pollutant concentrations between 2000-06. The fact that our model predictions are ∼30% lower than measurements is not surprising given the biases in the meteorological predictions discussed above. We believe this level of performance is typical or better than typical for downscaling exercises where observations cannot be used to constrain the behavior of the meteorological simulations over complex topography.

Comment 2: “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

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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."

Response 2: Currently the UCD/CIT airshed model does not incorporate any feedback mechanisms for clouds in the calculations of gas and particle phase concentrations. A similar approach (without feedbacks) has been adopted by virtually all groups studying long-term simulations since the computational expense of modeling feedbacks is currently prohibitive. This general limitation of the field (i.e. you can have feedbacks or long simulations but not both) will be stated in the revised manuscript as requested by the reviewer.

Comment 3: "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 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."

Response 3: The parent climate model PCM was run in the mode suggested by the reviewer. PCM simulations were generated over a continuous period from 1995-2099, with CO_2 increasing by 1% per year under the business as usual (BAU) B06.44 emissions scenario. Past studies have shown that fine-downscaling using meso-scale meteorological models like WRF enhances small scale meteorological solutions but does not significantly alter regional average behavior (Leung et al., 2004; Lian et al., 2006). The WRF downscaling exercises are initialized by the PCM output and then constrained by the PCM boundary conditions, and so they resolve the fine-scale details associated with the PCM results. It is not necessary to simulate the full period at high resolution using WRF in order to study the effects within shorter time intervals – this would require a tremendous computational burden without changing the outcome.

It is also unnecessary to simulate the air quality system for the full study period since the initial conditions quickly wash out of the air basin and pollutant concentrations generally respond to the emissions boundary conditions after $\sim$4 days of simulation time. This reflects the fact that the air quality system is inherently stable, not subject to random behavior influenced strongly by initial conditions.

The intent of the statistical analysis in the current study was to test whether the change between the future (2047-53) and present-day (2000-06) annual average PM2.5 concentrations caused by changing climate was significant. This is fundamentally different than studies seeking to determine if changes to greenhouse gas emissions influence climate. We believe that our analysis approach addresses the question we seek to answer appropriately, and that the use of longer simulation times for the WRF+air quality analysis would not change our conclusions. A summary of these points will be added to the revised manuscript.

Comment 4: "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."

Response 4: Zhao et al. (2010) showed that the future higher temperature rise over the ocean compared to the land is a feature of the PCM data during the winter season. PCM predicts that cloud cover and precipitation will increase during the future winter months over California land areas, leading to less temperature increase over land compared to over the ocean. Summer temperature trends followed a more conventional pattern with greater temperature increase predicted over land vs. over the ocean. The WRF down-scaling did not change the overall trends predicted by PCM, consistent with the notion that meso-scale models refine details but do not completely alter regional patterns.

An analysis of output from the CCSM global climate model between the years 2000-2006 and 2047-2053 also revealed higher temperature rise over ocean compared to land during the winter months. Consistency between the PCM and CCSM results builds confidence that the PCM results are plausible. Furthermore, it is not advisable to reject GCM results because they do not match historical trends. For example, an analysis of historical trends shows that most of the temperature increase in California to-date has occurred because nighttime temperatures have increased, not because daytime temperatures have increased (Duffy et al., 2006). The majority of GCM's break with this historical trend and predict that future daytime temperatures will increase over California. Thus, consistency between future predictions and historical trends is not a requirement for GCM output.

We agree with the reviewer that the pattern of temperature change is an important feature of the analysis, and we will modify the paper to state that CCSM results are currently being downscaled so that the air quality analysis can be repeated using another GCM as the starting point for the calculation. These results will be discussed in a follow-up paper.

Comment 5: "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."

Response 5: The title of the manuscript was carefully chosen to use the phrase "7 year analysis periods" rather than "7 year simulation periods". Never-the-less, we agree that the abstract can be modified to more clearly state that 1008 days were simulated within each 7 year analysis period to span a climatologically relevant time period with a practical computational burden.

Comment 6: "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 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".

Response 6: The submission date of the current manuscript was December, 2009 and the Jacobson study mentioned above was not available at that time. We will be happy to cite the new study, and put it into proper context with the current work. Note that Jacobson considered the effects of local CO2 forcing on air quality, which is different

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than global climate forcing on air quality (although they share a common cause: CO_2 emissions). The two studies are not in conflict – they seek to answer different questions using techniques that are appropriate for those questions.

Comment 7: "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? "

Response 7: The UCD/CIT model includes major atmospheric processes acting on gas and particle phase species including emissions, advection, dispersion, dry and wet deposition/removal. The model also includes chemical transformation and gas-particle conversion processes. Extinction of UV by airborne particles modifies the photochemical reaction rates. However, the model does not handle feedback mechanisms whereby emitted aerosols influence the temperature structure of the atmosphere or the formation of clouds. The description of the UCD/CIT modeling framework was included in the method section of the current study but only those features that were updated relative to previous work were discussed. A summary of all the processes that are included/missing in the UCD/CIT model will be added in the revised manuscript.

Comment 8: "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."

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Response 8: The ENSO cycle is generally ∼3-8 years, and so any effect from the ENSO cycle on air quality should to be present in a 7-year analysis period. Fig. 5 of the current study shows how the concentration of PM2.5 total mass varies from the 7-year average in the present-day (2000-2006) and future (2047-2053). The variation in a given year is primarily due to natural intra-annual/seasonal variations caused by the ENSO cycle. The El Nino phase of the ENSO cycle can be identified in California by increased winter precipitation. The year with the largest negative anomaly from the 7-year average had average precipitation rates that were ∼30%-90% greater than the 7-year average, proving that this was indeed an ENSO effect. A summary of this analysis will be added to the revised manuscript.

Comment 9: “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 does not predict feedbacks to the ozone layer, and this is another potential source of uncertainty.”

Response 9: This limitation of the current study will be addressed in the relevant section of the revised manuscript.

Comment 10: “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?”

Response 10: The wet deposition is calculated based on solutions of washout rate which depends on rain rates, rain-drop size distributions, and airborne particle size distributions. The gas-phase removal is calculated based on the rain rates and chemical composition of the gas-phase species (i.e., solubility). This description will be added to the revised manuscript.

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Zhao, Z., et al., The impact of climate change on air quality related meteorological conditions in California – Part II: Present versus future time simulation analysis. submit to J. Climate., 2010b.

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