

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: When talking on climate impacts using model downscaling I would expect the rigorous RCM strategy using driving GCM fields only for driving boundary conditions of the nested model and uninterrupted run for whole periods studied. The strategy of 17 days (finally -4 for evaluation) of simulation (initialized from coarse resolution data) and then 25 days skipped as my opinion does not allow the development of high resolution local feedbacks and patterns of surface processes, hydrological cycle etc. For RCM usually spin-up of at least a year is recommended. The averaging across those 9 time segments could be even affected by their selection, i.e. is there the same

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number of days in all seasons? Maybe at least say 15 days of each month to simulate and others 15 skipped to capture more reliable the annual cycle. When applying this method on present climate, it is rather close to the air-quality regional reanalysis concept, but for that purpose probably no need to run 17 days.

Response 1: The days that were simulated for each year were consistently the same for both the present (2000-2006) and future (2047-2053) periods, except for the leap/non-leap year difference. The break between the seasons is somewhat arbitrary and these break points may shift in the presence of climate change. Our method of uniform spacing throughout the year introduced less noise/randomness in the final dataset while keeping the simulation expenses at a reasonable level. Ideally, simulating all days in each of the seven-year periods would have produced the most complete dataset, but it would have been virtually impossible to carry out this strategy over a climatologically relevant time period using the spatial scales necessary to resolve California's air basins.

Zhao et al. (2010a) studied the performance of the WRF model in California using various configurations and they determined that the optimal land surface scheme was the Dudhia thermal diffusion parameterization that is based on vegetation type, without explicit consideration of soil moisture. Use of land surface schemes that explicitly consider soil moisture such as the NOAA LSM yielded considerably degraded performance for ambient relative humidity in California. As a result, the only direct connection between the hydrology cycle and air quality is the direct washout of pollutants during precipitation events, and this effect is properly captured in our simulations. The use of longer WRF modeling periods to predict changes to soil moisture would not affect any feature of the simulation. It would be highly desirable to construct a land-surface scheme that properly represents soil moisture in California so that these feedback effects could be studied in this region. The development, testing, and application of a new soil moisture scheme are beyond the scope of the current study.

Changes will be made to the manuscript to explicitly describe the problems associated

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with modeling soil moisture in California using WRF. The authors believe that the findings that the inter-annual variability plays the most important role in the climate impact on air quality would not change, even if all the days in each of the seven-year periods were simulated.

Comment 2: It is not clear whether the present climate simulation uses observed SST or ocean data from coupled GCM.

Response 2: A brief discussion on the meteorological modeling has been presented in the methodology section of the current study. A detailed description of the PCM-WRF modeling system and rigorous analysis of the model performance have been presented elsewhere by Zhao et al. (2010a). Two manuscripts regarding the PCM-WRF down-scaling have been submitted recently to the Journal of Climate by Zhao et al. (2010a,b).

Observed sea-surface temperature (SST) data are sparse over the Pacific and not readily available. Moreover, the current study involved down-scaling of the PCM data for the future (2047-2053) as well as the present-day (2000-2006) and so observed SST data could not be used. In order to be consistent, the SST data were directly obtained from the coarse resolution PCM output and interpolated to the fine resolution model domain for both the present-day and future simulations. The finding that the air temperature above the ocean is likely to be warmer than the air temperature above the land in the future comes directly from the PCM data. The authors also compared this PCM prediction with CCSM predictions and found consistent behavior for the years 2000-06 and 2047-53. Both models predict warmer temperature over the ocean compared to over the land for these time windows.

Comment 3: The chemical boundary condition in the regional simulation should be taken from some outer model as well to provide the proper climate signal, this is not clear from the paper.

Response 3: The same seasonal boundary concentrations (BC) of several gas and

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particle phase species were used for the present-day and future simulations in the current study, except for ozone, which was increased for the future simulations according to estimates published by Vingarzan (2004). The authors believe that the contributions from BC to the PM mass in the future would not be statistically significant due to large inter-annual variability.

Comment 4: I understand the effort to keep the simulation within some framework of the resources, but without the assessment of the impact of the criticized simplifications, e.g. by comparison of the results, time series etc. from both methods for at least a year of simulation (after proper spin-up) further statistical analysis of air pollutants concentration become a bit vague. The similar full RCM study with coupled CTM in quite high resolution of 10 km in Central Europe has been performed by Halenka et al. (2008, 2010a, b) assessing of climate change impact on air quality for decadal time slices 2041-2050 and 2091-2100 against 1991-2000.

Response 4: The limitations of the current model including the lack of feedbacks where aerosols modify the temperature structure of the atmosphere and/or modify cloud properties will be addressed in relevant sections of the revised manuscript. The challenges associated with model application in California's extreme topography, varied climate zones, and long-term ENSO signal are severe.

The conference proceedings describing the application of climate downscaling and air quality analysis by Halenka et al. will be cited in the revised manuscript. These results are not generally available by web of science search. Halenka et al. are encouraged to publish these results in more generally accessible peer-reviewed journals.

Comment 5: It would help for understanding if clearly stated both in abstract and the methods description that there is off-line coupling with no feedback of the chemistry to climate simulation concept applied. Similarly, to avoid confusing, it should be clearly pointed out in the abstract and the description that only climate effects are considered, that means that baseline 2000 emissions are used for future, as finally the reader can

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find in the Summary. However, is that valid for biogenic emissions as well or do they account climate change signal (temperature, radiation, etc.)?

Response 5: A brief description of the emissions processing have been presented in the methods section. The emissions for the present-day (2000-2006) and future (2047-2053) were based on the year 2000 emissions inventory so that the climate impact on air quality could be studied. The emissions from automobiles and biogenic sources were adjusted for temperature and radiation to reflect climate effects on those base emissions.

Comment 6: As for the periods chosen and their relation to ENSO, if I accept the period of ENSO could be 3-8 years as mentioned in the paper, then 7 year time slice might have problem in this region, e.g. when having two El Nino phases and one La Nina, which could be discussed based on GCM simulation or from observation if present run driven by observed SST. By the way, from 2000-2006 conditions it is clear that prevailing warm episodes appeared (http://www.cpc.ncep.noaa.gov/products/analysis_monitoring/ensostuff/ensoyears.shtml).

Response 6: We agree that it would be desirable to extend the analysis period to 20 years or more in order to capture several ENSO cycles. This would likely produce even greater amounts of inter-annual variability, although the extended analysis times would decrease the standard error as the square root of n (=number of sample days) will increase. We believe that the conclusions of our study would be consistent over this longer simulation time. The change in California PM_{2.5} concentrations between 2000 and 2050 is small relatively to the inter-annual variability within any 7 year analysis period.

Comment 7: No doubt it is legitimate to use whatever GCM, especially when contributing and compared within the IPCC assessment. However, some information on PCM validation as well as the validation of the downscaling system should be given. As one can see at <http://www.climatewizard.org/tnc/FutureClimateModels.html>, PCM climate

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signal for the region underestimate the temperature change through all the year and all the scenarios presented (B1, A1B, A2), precipitation signal is overestimated except for summer in A2 scenario with respect to the ensemble of IPCC AR4 models while CCSM model is far closer to this ensemble. Then one could doubt what the consistency of PCM with CCSM mean, the difference between the A2 and business as usual scenario in GHG concentration for mid-century won't be so big I would say.

Response 7: We realize that PCM predicts a cooler, wetter future in California relative to some of the other GCM's that have been applied. We are currently in the process of repeating our analysis using CCSM. The results of that analysis will be reported in a separate paper.

Comment 8: As for the excess of ventilation predicted used for accounting for under-prediction of PM_{2.5}, as my opinion not necessarily this has to occur both in present and future conditions. It might depend on circulation types changes as well which would need more detailed analysis.

Response 8: Zhao et al. (2010a) determined that the over-estimation of surface wind speeds is a persistent bias introduced into the simulation by the WRF model under most meteorological conditions studied for California. We believe that this feature affects present and future simulations equally, although it may mask more subtle effects. This point will be clarified in the revised manuscript.

Comment 9: In discussion of results presented in Fig. 3 the authors confuse the reader when mentioning predicted concentrations describing the present state while throughout the text this means future period.

Response 9: The figures and captions will be updated in the revised manuscript to be consistent.

Comment 10: There is no information on the other outer domains of WRF (size, height, : :) in the text or/and in the scheme.

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Response: The details of the WRF configuration are provided by Zhan et al. (2010a,b) and will be summarized in the revised manuscript.

Comment 11: All figures and legends, axis descriptions etc. seems to me mostly to be too small for clear viewing both in the maps and graphs, in graphs of Fig. 4 it is not easy to distinguish between modeled and observed results.

Response 11: The size of all figure captions will be revised to increase font size.

Reference:

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Leung, R., Y. Qian, X. Bian, W. M. Washington, J. Han, and J. O. Roads: 2004: Mid-Century Ensemble Regional Climate Change Scenarios for the Western United States. *Clim. Change*, 62, 75–113.

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