Atmos. Chem. Phys. Discuss., 12, C2039–C2043, 2012
www.atmos-chem-phys-discuss.net/12/C2039/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying population exposure to airborne particulate matter during extreme events in California due to climate change" *by* A. Mahmud et al.

Anonymous Referee #2

Received and published: 2 May 2012

In this paper the authors presented the effect of a climate change on PM2.5 concentrations over California. They dynamically downscaled the results of a global simulation for a present and future climate 'business as usual' scenario. They calculated particulate matter concentrations with a regional air quality model. The authors focused their analysis on the differences in annual average population weighted PM2.5 concentrations and the impact on extreme PM2.5 concentrations due to a climate change scenario. The presentation and the analysis of the results are generally clear, but it can be improved to clarify some of the methods and the analysis, I would suggest acceptance of the paper after taking into consideration the following comments.

General comments:

1) In section 1.1 some of the descriptions should be improved for the readers who are not familiar with the models described by the authors. In particular a better description of the BC and IC (page 5885 lines 28–29, 5886 lines 1-2) should be available in the text and not simply refer to a previous publication of the authors. A short description and an explanation of the term "source oriented" photochemical model should be provided (page 5586). How the model describes aerosol chemical composition and size distribution?

2) In Section 1.2 PM2.5 annual average concentrations simulated by the model over California are presented without showing any comparison to measurements. An evaluation of the model in reproducing PM2.5 concentrations and/or some of its components (sulfate, organic carbon, ..) should be presented or cited and summarized if done in previous publications. It should be also shown if the simulation, which covers only the 40% of each year, is able to represent the observed seasonal and inter-annual variability. For example it is claimed at page 5889, line 20, but the authors did not show or cite any previous paper to demonstrate this.

3) The effects of climate change on annual mean and extreme PM2.5 concentrations are discussed without a description of the changes in climate which may affect the chemical composition, accumulation, removal of aerosols in the atmosphere. The authors in Section 2 generally refer to increasing stagnation and increased annual averaged wind speeds. A description of the changes in the climate conditions should be better addressed, in particular in relation to the processes which are determining aerosol concentrations and secondary aerosol formation (for example, sulfate production from SO2 oxidation).

Minor comments:

Page 5882, in the abstract, when the term 'extreme event' is used, it is not always clear if it is related to extreme pollution events or extreme meteorological conditions which

determine high pollution events.

Page 5883, line 10, Samet et al., 200; Correct reference year.

Page 5883, line 16, "2.3 times higher than the NAAQS". Maybe would be good to insert the NAAQS limit here in parenthesis.

Page 5883, line 20, maybe an error here 'trapping leading'

Page 5884, lines 16-23, the authors claim here and also later on page 5889 that the present climate simulations (1000 days for 7 years) is able to capture the inter-annual variability. But they don't show if the PM inter-annual variability is well represented by the model. See also comment 2).

Page 5885, line 4, PCM data . . . were dynamically downscaled . . .

Page 5885, line 6-7, the global simulation with PCM was driven by nudging the present climate meteorology or only SSTs were prescribed?

Page 5885, line 12, is not very clear why the authors say "unbiased sample".

Page 5885, line 12, in the previous sentence they say 153 (17*9) days per year, which is a total of 1071 days, while here they say only 1008 days.

Page 5885, line 16-17, the vertical resolution of the AQ model is rather coarse, only 10 levels from surface up to 5 km. In such setting, the authors should show how well the model is able to reproduce observed PM and/or gas (e.g SO2 and other aerosol precursors) concentrations.

Page 5885, lines 18-25. When possible the authors should indicate a reference for the emission inventory and for the EMFAC and BEIGIS models.

Page 5885, lines 26-27. I think that would be helpful to shortly describe here the IC and BC conditions, even if a previous work is cited. When simulating a period of 17 days, a spin-up period was performed?

Page 5886, lines 3-13. The AQ model should be described with more details on the aerosol description: Chemical composition, size distribution, internally -externally mixtures? What does it mean exactly "source-oriented" model? I think this is important also to understand for example Table 1, where concentrations of some aerosol species are provided together with aerosols from specific emission sources.

Page 5886, line 23, is the population homogeneously distributed within the single basins, or is it distributed according to the main urban areas?

Page 5887, line 6-7, a statistical test (e.g t-test) was applied to determine if the two (future and present climate conditions) pm2.5 distributions have significant different averages?

Page 5887, line 13, I would remove '. . . x in the range of . . .'

Page 5888, lines 1-6. I think that in this session a comparison with measurements could be described to know how well the model can simulate PM concentrations (total pm2.5 or single chemical species, like sulfate or organic carbon).

Page 5888, lines 12-13, "90% confidence interval for the mean difference". It is not very clear how the average differences and the CI are calculated. In the discussion (page 5892 line 12) it is suggested that statistical tests were performed to verify if the differences were significant, but in the methods and here it is not clearly mentioned what kind of test is done (t-test?). Also to help the visualization of Figures 2, S1 and S2, the statistically significant differences could be highlighted with a different color or in bold.

Page 5889, lines 20-21, the authors claim that the inter-annual variability (of the meteorological fields/ chemical fields?) is captured but they do not show or cite previous papers to support this. See also previous comments.

Page 5890, line 10. Fig.S3, the tails are not easy to see due to the scale of y-axis. The authors could consider showing only the tails in the same or another figure.

Page 5890, line 14, some references would be needed here on extreme concentrations and public health relationships.

Page 5890, line 17, Fig.3, I would use the same scale for panels a and b, to highlight the differences between future and present-day simulations.

Page 5891, lines 10-13. If I well understood, the EVT was applied to extend the dataset of extreme values, which were used to calculate the 10 year return value with the GPD analysis. Maybe a more detailed description of this point (EVT) is needed, as it is also never mentioned before.

Page 5891, lines 19-21. The authors should explain with more details why they observe a decrease in sulfate and increase in ammonium nitrate.

Page 5891, lines 21-24. The increased stagnation is indicated as a cause of higher PM2.5 values, but this is not supported by further discussion. The selected extreme pollution events, are really in correspondence of stagnation period, and how this periods are identified in the simulations? Are the extreme events all characterized by similar meteorological conditions? See also the general comment 2).

Page 5892, lines 8-15, it is not clearly described in the previous sections if a statistical test (t-test?) was applied to determine if averages of the 2 distributions are different.

Page 5892, lines 23-25, increased wind-speed is indicated as the main reason for EC and OC reduction in annual averages of PM2.5. Wind speed changes are not shown in the previous section, and why increased wind speed should reduce only EC/OC from primary sources and not the other PM species?

Supporting information: lines 48-54, the numbers in the text do not correspond to Figure S5 (neither S3). I would also suggest improving quality of Figure S1, S2, S4 and S5, the numbers of each column are overlapping with the vertical bars.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 5881, 2012.