Atmos. Chem. Phys. Discuss., 12, C2000–C2004, 2012
www.atmos-chem-phys-discuss.net/12/C2000/2012/

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 #3

Received and published: 1 May 2012

General Comments

The paper looks to demonstrate a relationship between PM concentration levels and climate change, with climate change influenced by annual increases in CO2 emissions. The authors implement a computational model applied to California as a whole, and three specific regions in particular, for a present-day case and a future case. The method seeks to isolate the impact of climate change alone on PM through holding emissions at 2000 levels. Additionally, analysis of various regional scales allows for the deconvolution of data-averaging. Comparisons are then drawn between the two timeframes for annual-averaged concentration, composition, and source. Further analysis is presented for the worst 1% of 24-hr averaged PM concentration. The authors conclude that the annual-averaged data does not statistically indicate a change in PM concentration on either spatial scale, especially due to the fact that the 90% confidence interval for nearly all data includes predictions of both increases and decreases in studied parameters. However, the extreme cases did indicate increased PM exposure for the population in some cases. Changes in composition and source were also predicted for the future scenario. The authors mention stagnation events in local meteorology as a major cause of the predicted differences. The distinction between the annual-averaged and extreme-averaged results presents valuable insight and is an interesting result. However, the authors may need to provide some supplementary text to emphasize the statistical significance of these results the underlying assumptions of the model.

Specific Comments

1. The authors mention that the CO2 emissions rate rises by 1% per year for their set-up of the future climate scenario. Is this the only change that's made to be indicative of climate change? Discussion elsewhere in the paper seems to indicate so. Is this sufficient to properly model the climate change?

2. The authors mention on a few separate occasions within their model description that it has been assumed that population density, population, and emissions levels are held at 2000 levels. The discussion would benefit significantly from a more thorough consideration of how realistic this assumption is. As currently described, the model appears then to have a stagnation in population and industry for a 50-year period. On a related note, the referenced Mahmud et al. 2010 paper does not seem to make this assumption; why have the authors adopted it in this work?

3. In particular, the authors justify this assumption as a method to isolate the effect of climate change. This is somewhat true; given the assumed annual rise in CO2, there can be some climate change effect modeled, but all other possible climate change

forcing effects are held to their 2000 level. Do the authors not feel that this limits their analysis to primarily the study of the long-term effects of current emission levels? Three related questions that may require careful consideration (and explanation) are:

a. If growth rates of species emissions are prescribed (and therefore known), could a study not then elucidate the effect of the climate change physics separately from the growth in emissions rates? Would statistical analysis, using these growth rates as independent variables, not be able to still provide some sense of total climate change forcing's effect on PM?

b. Eventually, this method would predict some near-steady state (after all, CO2 is rising, but there may be a saturation point in its effect on PM), given a long enough simulation timeframe. Is there any evidence that the climate is approaching some steady-state in the context of the pertinent variables discussed in this work? Do the authors foresee any potential change in the nature of their results if growth was included? For example, does the growth in emission rate of some species x alter the nature of the predominant reactions in the atmosphere such that the reaction set enters a different "regime?"

c. How realistic is this in the context of observable and expected growth? Can a regulatory agency make any recommendations based on an assumption of zero growth?

4. The results for shipping sources in Figure 2 are surprising in the context of current observable trends, especially in SoCAB. The industry expects major increases in the activity of the Ports of LA and Long Beach in the years included within this study. However, this work shows a major decrease in contribution of PM from this source. It is well-known that ships at the ports are major contributors to PM emissions, and CARB has found this emission to be predominantly PM2.5, putting these ideas at odds with the authors' results. Can the authors provide any insight on this discrepancy?

5. The authors mention the 3rd and 4th Assessment Reports of the IPCC. Some quantitative comparison between the current work and these references would be helpful in understanding the new contribution of this work.

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Interactive Comment

6. The authors mention stagnation events being stronger in the future scenario. This is not shown in the Tables or Figures. A more thorough discussion, and visual demonstration, of these events will solidify the connection for the reader. As currently written, the reader must simply take the authors' word that these stagnation events occurred with greater severity in the future scenario. A further step would be for a statistical analysis to provide a quantitative measure between stagnation event severity and maximum PM concentration (or whatever similar measures the authors feel most appropriate).

7. The data in Figure 3 does not seem to be in agreement with S3. The maximum concentrations in S3 seem to be cut off at thresholds approximately 1/2 as high as those shown in Figure 3. Data seems to indicate that in worst regions, $\sim$50% increase in PM. Given uncertainties previously discussed by the authors, how should the 50% difference be interpreted (is it significant even with the wide limits on the 90% CI)? How does the smaller sampling (since this is only the top 1% of days now) affect the uncertainty and 90% CI?

8. A fundamental question that the authors should address is the choice between comparing current climate impacts on PM to the effects either in the past or the future. As an implicit basis of the work is that the climate is (and has been changing). With this assumption, is it possible to develop a more statistically significant (possibly more accurate) comparison if observed climate conditions from the past are used instead of projections to future conditions? After all, given the assumptions of the work and the high uncertainty in the current work, it does not seem that the authors are trying to answer what future climate will do to PM. Rather, the aim seems to be whether or not climate change can have regional PM effects. If this is the case, then the authors have freedom to choose the past or future to investigate.

9. The authors do not mention in their paper any possible feedback between PM emissions (dependent variable) and climate change (independent variable) itself. It is of course known that, depending on the composition of the PM, there can be a feedback provided to regional climate forcing. Do the authors have some evidence to show that

this effect can be ignored in their work or in general?

Technical Comments

1. The authors have slightly differing versions of their paper's title in the full manuscript, the supplemental material, and the submission paperwork.

2. Page 3, Line 10: The year is incomplete for the Samet et al. reference.

3. Page 3, Line 21: The phrase “trapping leading” seems to indicate a word or phrase is missing or one of these words is not intended.

4. Page 3, Line 26: The reference for Kleeman has an extra digit in the publication year.

5. Page 4: Line 2: “United Stated” is a typo.

6. Page 4, Line 19: “El Nino” requires the proper ñ character.

7. Page 4, Line 21: It is early in the paper, but the authors would benefit from using a more technically precise terminology than “tails” to describe the limits of the statistical distribution.

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