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Interactive comment on “Air pollution and associated human mortality: the role of air pollutant emissions, climate change and methane concentration increases during the industrial period” by Y. Fang et al.

Anonymous Referee #2

Received and published: 29 October 2012

This paper quantifies that premature PM_{2.5} and ozone related deaths resulting from total industrial pollution, and the impacts of emission, climate, and methane changes specifically. This is a comprehensive paper that is generally well written. However, there is not enough information in the paper to evaluate the health impact assessment methodology. There are also additional major concerns that need to be addressed.

Major issues:

- The health methodology is unclear and may contain serious misjudgments. The

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authors refer to a submitted paper for more detail on the health methods. Is that paper available or can the authors make it available to the reviewers to judge the health methodology?

Although it is difficult to understand, it seems that the authors quantify respiratory mortality for ozone but all-cause mortality for PM_{2.5}. At this point, the health literature has been able to discern more about how PM_{2.5} affects health than was known at the time of the first American Cancer Society publications (including Pope et al. 2002). It is now widely appreciated that PM_{2.5} is most strongly associated with cardiovascular effects, while ozone is more strongly associated with respiratory effects. I agree with the authors' choice to apply long-term respiratory mortality relationships for ozone based on the results from Jerrett et al. (2009). However, I have some issues with the PM methodology. While it may be true that cause-specific mortality data from many parts of the world may be inaccurate, the authors are then using baseline mortality rates for respiratory disease to quantify ozone mortality. In addition, they are extrapolating these relationships for the US to the rest of the world where people are dying of very different diseases, all of which get lumped into "all-cause mortality." This is a majorly flawed assumption – in my opinion less defensible than trusting the cause-specific baseline mortality rates (which by the way are used by the WHO in the Global Burden of Disease study along with many other high-profile scientific assessments). Finally, the authors need to provide guidance on whether the all-cause PM and respiratory ozone mortality can be summed together or whether there may be some overlap between the two.

In addition to making the methods contained within the submitted paper available, to allow the reviewer to judge the health methodology it is imperative to provide more information on the range of ozone and PM concentrations simulated by the model for each scenario (by region even better), the PM species included in the PM definition (are dust and sea salt included?), population-weighted average change in concentration for each set of scenarios compared, and the regional population and baseline mortality rates used to do the health quantification.

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In general, it would be useful to describe the health methods inside this paper in more detail as ACP readership may not be familiar with them. At the very least, the relative risk estimates should be noted in the text, not just in Table 2.

- I have concerns about the results of the model evaluation in Section 3. The authors state that bias over populated areas in the US and Europe ranges from 4-10ppb and correlation coefficients are >0.7 in the US and >0.95 in Europe. Figure 1 shows that bias in the Northeast and Southeast US is around 14ppb, higher than the range given in the text for populated areas in the US. Some of the biases in Europe also reach much higher than 10ppb. These results do not demonstrate to me that the model is predicting ozone concentrations well for the locations where we have monitored observations.

In addition, the text description of the PM evaluation does not do justice to what looks like an extremely low bias and substantial variation in the direction of that bias in locations in the US, Europe, and Asia. As with ozone, these results do not demonstrate to me that the model is predicting PM well for the locations where there are monitored observations. In addition, many of the relative difference values in Europe and Asia look like they are overpredictions (i.e. red), but there are very few overpredictions shown on the scatterplot. Are some of the data not shown on the scatterplot or am I misinterpreting the values in the map? Regardless, more explanation is needed as to what the scatterplot and map depict, and how they should be interpreted for this study.

- Why are preindustrial PM concentrations so high? Can these values be set in the context of other model studies using the same emissions inventories (i.e. other models running ACCMIP emissions)? Previous studies with which I am familiar show preindustrial PM very close to zero. Perhaps the PM_{2.5} definition includes dust and sea salt, which have been excluded by many studies in the past, including Anenberg et al. (2010)? Please describe in the text what is included in the PM definition and consider whether your definition influences comparisons made to Anenberg et al. (2010). If dust and sea salt are excluded, the degree to which this affects the conclusions regarding relative importance of emissions, climate, and methane depends on why preindustrial

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PM is so high.

- There needs to be much more comparison of your results with previous studies, for both the effects of climate on PM and ozone and the health results. Please put the results showing the climate impacts of PM in context by discussing the conclusions of Jacob and Winner (2009), who note that the literature is inconclusive with regard to the directional impact of climate on PM. The impact of methane on ozone-related mortality could be compared with results from West et al. (2006), Anenberg et al. (2009), and Anenberg et al. (2012).

Jacob, D.J., Winner, D.A. (2009) Effect of climate change on air quality, *Atmospheric Environment*, 4: 51-63.

West, J. J., A. M. Fiore, L. W. Horowitz, and D. L. Mauzerall (2006) Global health benefits of mitigating ozone pollution with methane emission controls, *Proceedings of the National Academy of Sciences*, 103(11): 3988-3993.

Anenberg, S.C., J.J. West, A.M. Fiore, D.A. Jaffe, M.J. Prather, D. Bergmann, C. Cuvelier, F.J. Dentener, B.N. Duncan, M. Gauss, P. Hess, J.E. Jonson, A. Lupu, I.A. MacKenzie, E. Marmer, R.J. Park, M. Sanderson, M. Schultz, D.T. Shindell, S. Szopa, M. Garcia Vivanco, O. Wild, and G. Zeng (2009) Intercontinental impacts of ozone pollution on human mortality, *Environmental Science and Technology*, 43(17):6482-6487.

Anenberg, S.C., J. Schwartz, D. Shindell, M. Amann, G. Faluvegi, Z. Klimont, G. Maenhout, L. Pozzoli, R. van Dingenen, E. Vignati, L. Emberson, N.Z. Muller, J.J. West, M. Williams, V. Demkine, W.K. Hicks, J. Kuylenstierna, F. Raes, V. Ramanathan (2012) Global air quality and health co-benefits of mitigating near-term climate change through methane and black carbon emission controls, *Environmental Health Perspectives*, 120:831-839.

Minor issues:

- P. 22714 lines 15-19: Explain the standard deviation values given for the concentration

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results or remove from abstract.

- P. 22714 line 26: Add “above preindustrial”?
- P. 22714 lines 26-28 and throughout, it seems odd to lump together climate and methane impacts, since the methane increases since preindustrial presumably are mainly driven by methane emissions. Hence, the methane impact really adds to the impact of the growth in the other emissions as the total impact of anthropogenic emissions since preindustrial. Suggest removing this sentence as it doesn't add anything beyond the preceding sentence anyway.
- P. 22715 line 11: This is a factor of 5, not 4.
- P. 22716 line 10: Anenberg is misspelled
- P. 22718 line 24: missing “.”
- P. 22720 line 3: “its” misspelled
- P. 22721 line 15: Anenberg et al. (2010) did not separate out IHD from cardiopulmonary
- P. 22721 line 21: missing “.”
- P. 22726 line 18: define somewhere whether biomass burning includes open fires only, or also ag burning and residential cookstoves
- P. 22726 line 20-22: This statement implies that anthropogenic emissions increase homogenously. . . not true, even if they do increase everywhere.
- P. 22731 line 2: J-value is not defined
- P. 22732 line 27: state whether the 15% increase is only valid for pop-weighted total ozone. Would it be a larger fraction of non-population weighted ozone, since methane influences ozone everywhere while shorter-lived precursors influence ozone more near populated areas?

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- p. 22736: Do you really need to define a new term for the mortality contribution? No information would be lost and it would read more easily if you eliminate section 5.2.4 with the new term and simply provide the percent contributions of each factor in 5.2.1-5.2.3 (which are currently very short).
- P. 22738 lines 17-18: Suggest rephrasing the reference to SRES and RCP as these have not been previously defined.
- P. 22750: Header for regions is mislabeled
- P. 22753: T-test results are useful, but it is very difficult to see the map underneath since the maps are so small. Is there another way to provide the same information without obscuring the maps? Figure 4 is easier to see since it is bigger

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 22713, 2012.

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