

Interactive
Comment

Interactive comment on “Impacts of global, regional, and sectoral black carbon emission reductions on surface air quality and human mortality” by S. C. Anenberg et al.

Anonymous Referee #2

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This manuscript evaluates the global/regional health impacts of halving black carbon emissions based on variety of sensitivity tests. The methodology is scientifically sound and the results are policy relevant. It supports additional motivations to mitigate black carbon emission which may have a large positive effect on global warming. I recommend it being published in Atmospheric Chemistry and Physics after the following major and minor issues have been addressed:

Major comments:

1. The key assumption of this study is the toxicity of black carbon is equal to that of PM_{2.5}, which could result in substantial uncertainties of the results. Us-

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ally epidemiological studies derive concentration-response relationships based on temporal/spatial changes of total PM_{2.5} mass. In section 2.3, the authors show some evidence that PM_{2.5} mixtures with high BC fractions have stronger associations with mortality than other mixtures, but comment on “evidence for differential toxicity of BC and BC-containing mixtures remains inconclusive”, and therefore “assume all mixtures of PM_{2.5} are equally toxic”. The discussion here is insufficient and needs additional analysis on whether or not the assumption is appropriate based on existing epidemiological/toxicological studies.

2. It is nice to have a discussion on the feedbacks of changing BC emissions on sulfate concentrations. However, BC aerosols absorb/scatter radiation, which not only influences photolysis rates, but also changes the lapse rate of atmosphere, properties of CCN, and therefore influences cloud and precipitation. The former may influence atmospheric circulation and the latter will increase/decrease aerosols' wet deposition. Currently, it is not clear whether or not these feedbacks are being included in this study and how they may influence the results.
3. Only annual mean surface observations are used to evaluate model results. How about the model performance on seasonal variability and vertical profile of BC concentrations? More model evaluations on BC are needed.

Minor comments:

1. Page 10655, “but all PM_{2.5} components are thought to be damaging to health”. Need a reference here.
2. Pages 10656-10657, need to describe how SOA is simulated in this study. Are there any mechanisms that the change of BC will influence the production of SOA?

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3. Middle of Page 10657, dry deposition is set to 0.1 cm s^{-1} . However, a number of previous studies use surface resistant method to simulate the dry deposition velocities of aerosols, and find dry deposition velocities could range from 0.02 to 0.8. The authors may comment on the effects of changing tuning variables (e.g., dry deposition velocity) on the final results.
4. Page 10658, second paragraph, sensitivity tests are made based on 50% reduction of anthropogenic BC emissions, including residential, industrial and transportation sectors. Given the fact that BC aerosols disturb photochemistry in the model, many other chemical fields will change as well. Therefore, there would be some non-linearity involved in the system, such as sulfate and SOA. The authors should briefly explain the reasons to halve BC emissions from science/policy perspectives (e.g., why not 20% or 80%?)
5. Section 4 “Sensitivity analysis”: may change to “Sensitivity analysis on CRFs”.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 10653, 2011.

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