Atmos. Chem. Phys. Discuss., 11, C6299–C6304, 2011
www.atmos-chem-phys-discuss.net/11/C6299/2011/

Atmospheric Chemistry and Physics Discussions

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.

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Received and published: 12 July 2011

Response to Anonymous Referee #2: We thank Referee 2 for suggesting useful changes that enhance this manuscript. Please see our responses to comments from Referee 2 below. Referee comments are in italics.

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:

Thank you – we hope our responses below adequately address your comments and believe the paper has improved as a result.

1. The key assumption of this study is the toxicity of black carbon is equal to that of PM2.5, which could result in substantial uncertainties of the results. Usually epidemiological studies derive concentration-response relationships based on temporal/spatial changes of total PM2.5 mass. In section 2.3, the authors show some evidence that PM2.5 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 PM2.5 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.

We have now expanded the discussion of differential toxicity of PM2.5 mixtures, and although we believe an extensive review of the literature is not appropriate here, we now reference a draft report by Industrial Economics Incorporated, written for the US EPA, on uncertainty analyses to support the second section 812 Benefit-Cost Analysis of the Clean Air Act, where this issue is discussed in detail. The revised text now reads:

"Some evidence suggests that air pollution mixtures with high BC fractions, "black smoke," "diesel PM2.5," and "traffic PM2.5," have stronger associations with mortality than other mixtures (Cooke et al., 2007; Brunekreef et al., 2009). Some studies that use ambient BC concentrations as a marker for air pollution mixtures also find stronger associations with mortality than those using total PM2.5 (Ostro et al., 2007, 2008; Bell et al., 2009; Peng et al., 2009; Smith et al., 2009). However, these studies are subject to large measurement and exposure error since BC is very spatially heterogeneous (Bell et al., 2010). Furthermore, many PM2.5 constituents are correlated, subjecting single-pollutant risk estimates to confounding by co-pollutants and often preventing

ACPD

11, C6299–C6304, 2011

definitive conclusions about their relative importance to risk (Smith et al., 2009). The body of evidence for differential risk of PM2.5 components, including BC, is not as robust as for long-term PM2.5 (Smith et al., 2009; US EPA, 2009). We therefore assume that all mixtures of PM2.5 are equally potent in causing premature mortality, and use the change in total PM2.5 in Eq. (2)."

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.

This is a good point. We use a chemical transport model (CTM) with meteorology as input to the CTM, rather than a coupled chemistry-meteorology model that would also account for feedbacks of chemistry on meteorology. However, our CTM does include a module that allows for feedback of aerosols on photolysis rates, but not on atmospheric dynamics. To clarify this, we added discussion to the Methodology and Discussion sections.

The following sentence in Section 2 on Methods was revised:

"An online photolysis scheme accounts for the impact of aerosols on photolysis rates, affecting production of photochemical oxidants (Tie et al., 2005; Emmons et al., 2010b), however, aerosol feedback on meteorology (including atmospheric circulation and interactions with clouds) is excluded."

The following sentence was added to Section 5 on Uncertainties:

"While we include feedback of aerosols on photolysis rates, we exclude the impacts of aerosols on meteorology, including on the atmospheric lapse rate, which would affect

ACPD
11, C6299–C6304, 2011

circulation, and cloud condensation nuclei, which would affect aerosol wet deposition rates."

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.

We focus here on evaluating annual average concentrations at the surface (as mentioned in the title of our paper), the quantities we report as concentrations and use for the health impact assessment in the results section. While evaluation of the vertical profile of concentrations is outside the scope of this paper, we plan to write a follow-on paper focusing on column concentrations and radiative forcing impacts, where we will consider evaluating vertical profiles as they are pertinent to those results. While monthly concentrations are not reported in this paper nor are they used for the health impact assessment, we agree on the value of including such an evaluation for BC and have added monthly comparisons with observations in the Supplemental Material.

The following sentence was added to the main text in Section 2.2:

"See the Supplemental Material for comparisons of simulated monthly concentrations with IMPROVE and EMEP observations (Figs. S9 and S10)."

Minor comments:

1. Page 10655, "but all PM2.5 components are thought to be damaging to health". Need a reference here.

Added reference to Krewski et al. (2009).

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?

In MOZART-4, SOA is linked to gas-phase chemistry through the oxidation of monoterpenes and toluene (Emmons et al., 2010). We would therefore expect SOA mass to be

ACPD

11, C6299–C6304, 2011

affected by the BC and BC+OC reduction scenarios, through the associated changes in oxidant concentrations.

The following sentences were added to Section 3.1:

"NO3 and SOA concentrations are also formed in the atmosphere by reaction with photochemical oxidants. We find changes in regional annual average NO3 concentrations up to 20 ng/m3 (0.3%) as a result of the BC emission reduction, but no appreciable change in SOA."

"We also find mixed directional changes in NO3 (regional increase up to 200 ng/m3, 2.0%) and SOA (up to 47 ng/m3, 55%) that do not necessarily follow the directional change in SO4 (Fig. 5b)."

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.

Added "and dry" to the sentence on aging and wet deposition uncertainties in Section 5 and the 2nd sentence below:

"Simulated BC concentrations vary widely among global CTMs due to differing assumptions for emissions and parameterization of aerosol processes, such as aging and wet and dry deposition rates (Koch et al., 2009; Vignati et al., 2009). Underestimation of deposition fluxes would cause overestimation of PM2.5 concentration and mortality impacts, and vice versa."

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

ACPD

11, C6299–C6304, 2011

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%?)

Added the sentence:

"The 50% reductions are chosen to simulate realistic but ambitious policy targets, while producing changes in PM2.5 that are sufficiently large to be analyzed in all reduction scenarios."

5. Section 4 "Sensitivity analysis": may change to "Sensitivity analysis on CRFs". Changed to "Sensitivity of results to concentration-response factors"

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C6299/2011/acpd-11-C6299-2011-supplement.pdf

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