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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Potential impact of a US climate policy and air quality regulations on future air quality and climate change" *by* Y. H. Lee et al.

Anonymous Referee #1

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1. General comments

The study uses a comprehensive AGCM-chemistry model to investigate how drastic measures to reduce US carbon dioxide emission by 50% by 2050 would affect both air quality and climate in the coming years. Furthermore, it incorporates air quality regulations together or separately from carbon emission reductions. While world leaders seek solutions to curb carbon emissions and prevent climate change during the Paris COP21 meeting, this type of articles comes well timed, in my opinion. The future of US climate and air-quality regulations is relevant, because the country is the largest historical climate polluter and US policies exerts global influence.

In my opinion a key message of the article is that US climate policy would generate air quality co-benefits, even if no direct air-quality measures are taken, this would be

already a clear win-win situation. Air quality regulations alone would have important health benefits, but PM reductions would lead to a global positive forcing, having negative climate consequences. If both air-quality and climate regulations are implemented, the health benefits are large and the negative climate effects from PM reduction is smaller than the positive climate benefits from carbon reductions on a global scale. I agree that US air-quality and climate regulations should be implemented together to address these two closely related environmental problems as the authors suggest.

In my opinion the study is interesting and novel, but I am still missing three important things in the it:

I. There doesn't seem to be any attempt in the manuscript to evaluate the model's ability to correctly simulate short-lived climate pollutants. The comparison between a one-moment aerosol scheme and a two-moment aerosol scheme is definitely very valuable as it provides a measure of the model precision, but by no means it yields the uncertainty in aerosol modelling as the authors suggest in the conclusions. To evaluate uncertainty, models should be compared to observations. Can the model simulate present-day PM2.5 and ozone concentrations? Has someone compared GISS ModelE2 with any of its two aerosol schemes with observations? I think a section discussing these key issues is necessary.

II. Carbon dioxide concentrations from future emission scenarios are derived by using a simple carbon cycle approach as in the IPCC reported in 2007. The manuscript shows no estimate of the uncertainty in future carbon dioxide concentrations estimated with that simple carbon cycle method approach. It would be interesting to learn how variable this estimates are and by how much could they affect the associated carbon dioxide forcing by 2050. I don't mean to run more simulations, but to discuss it in openly the manuscript, since CO2 is the main climate forcer after all.

III: I think the article should clearly acknowledge that the study is based in numerical model experiments and openly discuss the disadvantage of running the same AGCM in

all experiments. This relates to point I. How well does GISS ModelE2 represent aerosol properties and the direct and indirect aerosol effects? Due to the policy relevance of the article the authors could motivate similar studies with different models.

2. Specific comments

Line 15 page 31386: Add “to” in “leading a strong positive forcing ...”

Line 20 page 31386 & Line 1 Page 31404: I don’t agree that having a regional positive forcing of 0.22 W/m2 by 2050 over the US is necessarily a climate dis-benefit, while the global radiative forcing is still negative. The number is relatively small and no climate effects (beyond the forcing) have been estimated in the article or even cited from other studies. Why is it a dis-benefit? Please clarify.

Line 23 page 31388: Remove on of the “the”s.

Line 15 page 31391: Describe a bit more what is not included in micropyshics in OMA model, I understand that deposition, both wet and dry is taken into account, but what about nucleation, coagulation and condensation/evaporation? I understand that they are not represented in GISS-OMA, having no effects on the aerosol size distribution. Is this correct?

Lines 15 to 20 page 31395: Why is valid using annual PM2.5 and hourly O3 concentrations to estimate mortality rates. Why not using hourly or daily PM2.5? Aren’t daily exceedences of PM2.5 also very important for mortality estimates?

Lines 8 and 9 page 31400: Are You sure that the main difference between models is due to the missing nitrate aerosol. What about aerosol microphysics in TOMAS vs. no aerosol microphysics in OMA. How important is it to have a microphysical treatment of aerosol particles in a AGCM? Since You have done similar simulations with both aerosol schemes, it would be very useful to have more discussion on the differences and maybe even a table or figure.

Figure 4: Why is the difference in PM2.5 not shown?

Figure 5 caption: What do the error bars mean?

Figure 6 caption: Do You really mean “upper error bars” or is it “lower error bars” ?

Figure 12 caption: Is it really “Same as Fig. 8” or is it “ Same as Fig. 9” ?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 31385, 2015.