Response to review #2 on acp-2015-1054

Co-benefits of global and regional greenhouse gas mitigation on U.S. air quality in 2050

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We thank referee #2 for the positive and constructive suggestions and comments, which have helped us improve the manuscript. All comments have been carefully addressed here (blue colors are for referee's comments), and we have tracked all changes in the revised manuscript.

The authors expand a previous study using dynamical downscaling and global emissions inventories in order to evaluate the impacts of global climate policy and climate change on regional air quality in the US, and compare these impacts to those modeled (in a consistent manner) at a coarser resolution. This study provides many valuable contributions including comparisons of: (1) regional vs global scale air quality co-benefits of consistent GHG emissions scenarios both with and without a climate policy (2) domestic vs foreign contributions of GHG emissions/policy to US air quality co-benefits (3) the effects of GHG policy on co-emitted pollutants vs changing climate on co-benefits. The study is ready for publication after a few minor revisions, assuming the units reported for the average change in rainfall amounts were a mistake.

**Response:** The reviewer is correct that the rainfall changes were wrong by miscalculation, and we have corrected this error in the new manuscript, Fig 1 (b), Fig. S2(b) and Fig. S3(b). Please see Pg 5 Lines 25-26: "Additionally, precipitation is projected to decrease over most of the U.S. in both scenarios with U.S. average decrease of 0.20 and 0.15 mm $day^{-1}$ in RCP8.5 and RCP4.5."

Given the importance of foreign GHG policies on US ozone, it would be helpful to see more information about the source of those rather large decreases in domestic ozone. There were two places where the paper suggested that global (but non-US) methane was the largest contributor to changes in domestic ozone, but it would be extremely helpful if that could be quantified, or discussed in more detail especially in light of the issue areas of the western US seems to be having with the idea of meeting more stringent ozone targets given these large contributions from "background" or "uncontrollable" sources. And if the reduction in methane concentration (as reported in Table 1) is indeed the largest source of the reduction of domestic ozone, what does that look like with respect to emissions? This finding was a big take-away from the paper and so more information about it from the policy perspective would be valuable.

**Response:** We agree that it would be useful to quantify the influence of global methane changes under RCP4.5 on U.S. ozone with extra sensitivity simulations. However doing so will require more simulations of both global and regional models. Considering the intense computational requirement, we are unable to perform these extra model simulations here. We suspect that methane emissions are very important because there is a large difference in methane between REF and RCP4.5. For the global co-benefits study (West et al., 2013), we tried to estimate the methane contribution by multiplying the change in methane by the present-day sensitivity of ozone to methane, which yielded a very large ozone change that was greater than the total change. The problem is that 2050 precursor emissions are lower than at present, and so the sensitivity to methane would also be lower. Without extra simulations, we cannot quantify this effect.

Methane is included in both the global and regional model as a fixed global concentration, but at the Referee's request, we have reported the global anthropogenic emissions that these correspond to (Pg 11 Line 11): "This large influence of foreign reductions for $O_3$ highlights the importance of global methane reductions in RCP4.5 (anthropogenic emissions of 330 Tg $yr^{-1}$ in 2050 in RCP45, compared to 432 Tg $yr^{-1}$ in REF), and air pollutant emission reductions particularly in Asia and intercontinental transport.

Can you also clarify why US methane is not included? Even if inventory suggests it is small.

**Response:** The US methane emissions are only a small fraction of the global $CH_4$ emissions (32 Tg $yr^{-1}$ in REF in 2050, 7.4% of the anthropogenic total). We didn't model the effects of methane emissions directly, but instead use fixed concentrations of methane in 2050 that correspond with the emissions in the different scenarios. It would be possible to calculate the change in global methane concentration associated with US methane emissions alone (or foreign emissions alone), but that calculation would be uncertain. Doing so would also require additional simulations with the global model MOZART-4 in addition to CMAQ. Instead we chose to treat methane emissions as entirely from foreign sources and acknowledge the small error involved.

To address the reviewer's concern, we rephrase the sentence in Pg 8 line 7-9:

"In each scenario, we fix global methane at concentrations given by the RCPs (Table 1), and account for methane changes as a foreign influence, neglecting the fraction of global anthropogenic methane emissions that are from the U.S. (7.4% in 2050 REF scenario and 7.0% in 2050 RCP4.5)."

There seems to be some inconsistency between the original spatial distribution of emissions from the global inventories and the spatial allocation that was used for regional modeling of both emissions and meteorology. You mention that a benefit to using the global emission inventories (versus projecting the NEI) is that they take into account changing land use. But wouldn't both WRF and SMOKE use land use data that is both constant between 2000 and 2050, and inconsistent with the global representation?

Also, it is not clear to me how the emissions downscaling methods you used would provide any additional spatial detail (greater than that provided at a 0.5 x 0.5 degree level)? It seems that for VOCs and PM, there is detail added by scaling un-speciated totals by the speciation profile of the predominant source in each grid cell? But if I'm understanding your methods correctly, for any species other than VOC or primary PM there actually isn't any improvement in spatial allocation? If that is true, this is a downside you should mention as it would essentially smear your emissions out to the global scale resolution. But perhaps I'm missing something, in which case, your paper would benefit from more clarity in this regard.

**Response:** We address these related comments together. The reviewer is right that we use constant (year 2000) land use and land cover in both the WRF and CMAQ simulations. This would lead to inconsistencies in the land use and land cover distributions and the spatial distribution of emissions. For CMAQ this would be important, for example, for biogenic VOC emissions. We are not aware of any downscaling study that simulates spatial distributions land use changes and anthropogenic emissions in a way that is completely consistent, and we are planning to do work that increases this consistency in the future.

For anthropogenic emissions, however, we account for future land use changes and their effects on the spatial distribution of emissions in the two scenarios. In the paper, we contrast our emission downscaling method with the traditional NEI scaling method, which multiplies the spatial emissions from the current NEI by a national mass ratio between the future emissions and the current NEI. By doing this, the traditional method assumes that the air pollutant spatial distributions in the future stay the same as the current NEI. In contrast, we use the projected RCP emissions datasets that include projected changes in the spatial distribution of emissions at 0.5 × 0.5 degree resolution, and regrid those to the CMAQ grid (36 km). Because we simply regrid the RCP emissions, our methods do not provide additional spatial detail beyond what is provided by the RCPs at 0.5 degree resolution. Our results show that the spatial distribution of emissions do change in the future (rightmost plots in Figs. S4-S10).

We have added text in Pg 6 line 10 to the paper to acknowledge the inconsistency in land use assumptions:
"We use constant (year 2000) land use and land cover for all simulations in WRF and CMAQ, whereas the spatial distributions of anthropogenic emissions change in the RCP scenarios."

To clarify, we add text in section 2.2 Pg 6 line 4:
"By doing this, the traditional method assumes that future spatial distributions of emissions stay the same as the current NEI."

We rephrase text in Pg 6 line 9-10:
"By regridding the REF and RCP4.5 data, we account better for changes in the spatial distribution of future emissions projected in the RCPs (Figs. S4-S10), but do not provide additional spatial detail beyond what is provided by the RCPs at 0.5 degree resolution,"

I was glad to see more details on the relative changes to different PM species as a result of the GHG policies and climate impacts, however, it was not clear why OM is the largest change? That would seem related to the changing climate, less so changing emissions, but changing emissions dominate so that doesn't explain what is going on with OM.
**Response:** The OM decreases are dominated by the decrease of primary organic carbon (POC, 0.074 μg $m^{-3}$ decreases). By adding new figures showing the OM component changes from emission reductions and climate (Fig S19 in the supporting info), we see that the POC decreases are mainly caused by emission reductions, and the SOA decreases by changing climate. To clarify, we add the following text in Pg 10 Line 30 "In Fig. S18, the OM decrease is caused mainly by primary organic carbon (POC, 0.074 μg $m^{-3}$ decreases), followed by biogenic SOA (ORGB, 0.057 μg $m^{-3}$) and non-carbon organic matter (NCOM, 0.048 μg $m^{-3}$). The POC and NCOM decreases are caused mainly by emission reductions, while the SOA decrease is caused mainly by changing climate (Fig. S19)."

Page 5, line 25: That unit can't be correct. ??
**Response:** We corrected magnitude change for precipitation in the manuscript, as well as Fig. 1(b), Fig. S2(b) and Fig. S3(b).

Page 6, line 18-19: This sentence is not clear. Why not use the spatial allocation data available through SMOKE, or is that what you mean here?

**Response:** This sentence refers to our method of back-calculating total PM emissions from BC and OC, and does not refer to the spatial allocation of emissions. When we downscale the RCP emissions for CMAQ, there are more than one sub-categories inside one sector, e.g., the sector "Industries" includes emissions from "1A2_2A_B_C_D_E" as listed in Table S1. SMOKE provides more detailed sub-category information for the emission sectors. So when we match the emission sectors between the global RCPs and SMOKE, there usually are more than one category, and the speciation cross-reference files are slightly different between each sub-category. When that happens, we use the speciation cross-reference file from the sub-category with largest mass fraction in this sector, following the methods of Reff et al. (2009) and Xing et al. (2013).

To make it more clearly, we revise the sentence in Pg 6 line 18-19:
"In back-calculating total PM emissions from BC and OC, there are usually more than one sub-category within one sector, e.g., the sector "Industries" includes emissions from the sub-category of "1A2_2A_B_C_D_E" (Table S1). When that happens, we use the speciation cross-reference file from the sub-category with largest mass fraction in this sector, following the methods of Reff et al. (2009) and Xing et al. (2013)."

Page 8, line 24-26: it seems you used median for both ozone and PM2.5? Can you justify?

**Response:** From a recent CMAQ evaluation paper (Appel et al., 2008), they suggested using "median" over "mean" when the species evaluated are not normally distributed, which is commonly the case for PM species. They also suggested if the data were normally distributed, the mean and median would be the same. We provided the right reference for this sentence in the new manuscript.

Page 9, line 6: These are switched, are they not? Over-prediction is higher for MDA8?

**Response:** Yes, and it should be "The overprediction is slightly lower for 1hr-$O_3$ than for MDA8-$O_3$". We fixed in the new manuscript (Pg 7, line 6).

Page 13, line 5-7: Seems there is an error in this sentence.

**Response:** We used spectral nudging in our WRF downscaling studies, and meant to compare here with "analysis nudging". We updated in the new manuscript Pg 13 Line 5-7:
"Spectral nudging is adopted in this study to restrain WRF from drifting from the GCM, which has been shown to be better for some meteorological variables, but analysis nudging better for others (Bowden et al., 2012, 2013; Liu et al., 2012; Otte et al., 2012)."