Response to review #1 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 #1 for providing thoughtful comments. We have responded to each comment below and have noted the page and line number for each revision to the manuscript. (blue colors are for referee's comments).

This study used dynamical downscaling to investigate the climate impact on air quality. Based on their previous work West et al. (2013), they applied relatively high resolution (36 km) to further explore some regional features of climate impact on O3 and PM2.5. The scenarios designed are interesting and I recommend its publication after addressing the concerns below:

The added values in regional models: In the world of dynamical downscaling, researchers try to improve model predictions using regional climate models despite intensive computational resources. It would be very useful if the authors show some comparisons between regional downscaled results and the driven GCM either in meteorology or air quality or both. For meteorology, the authors show changes in Figure S1 to S3, but we are not sure which one performs better. Similar question applies to air quality.

Response: We agree that the comparisons between the global models and downscaled regional models are very important to justify the downscaling work. We have added new analysis to the paper to provide more complete comparisons of both the global and regional models with observations. In Fig. S1, we add comparisons of 2-m temperature (T2) and precipitation for both the GCM and WRF results with observational data. We compare the T2 modeled by GFDL AM3 and WRF with 21 years of observations (1979 to 2000) from the North America Regional Reanalysis data (Mesinger et al., 2006). We also compare the precipitation from GFDL AM3 and WRF with 41 years of observations (1948 to 1998) from the Unified US Precipitation data products from NOAA Climate Prediction Center (Higgins et al. 2000). We see that the WRF downscaling helps to resolve important features that influence the average regional climate that are not resolved by GFDL AM3.

We rephrase the sentences in Pg 5 line 18-24 to show these changes:
"We compare the downscaled WRF and the global GFDL AM3 simulations (for three-year averages instead of four to be consistent with CMAQ outputs below), for 2-m temperature (T2) with 21 years (1979 to 2000) of observation data from the 32-km North America Regional Reanalysis (NARR; Mesinger et al. 2006), and for precipitation with 41 years (1948 to 1998) of observation data from the 0.25° × 0.25° Unified US precipitation data product from NOAA Climate Prediction Center (Higgins et al. 2000). The large-scale spatial patterns for both T2 and precipitation between WRF and GFDL AM3 are similar (Fig. S1). However, the downscaled simulations help resolve important features that influence the average regional climate, such as those related to topography."

For air quality, we added new comparisons between MOZART-4 and CMAQ for the simulated 2000 $PM_{2.5}$ and O_3 (see the new Fig S11 in the supporting info). We see that the CMAQ is better in capturing the urban scale air quality than MOZART-4. We add the following text in Pg 9 line 7:
"By comparing the simulated annual $PM_{2.5}$ and O_3 in 2000 (both are three-year averages) between MZ-4 and CMAQ, we see that CMAQ captures urban scale air quality better than MZ-4 (Fig. S11)."

We also show in Fig 2 that the future $PM_{2.5}$ changes for RCP4.5 in 2050 relative to 2000 (also see Fig S16 for comparison between REF in 2050 relative to 2000), are within the ACCMIP ensemble model means for both the MOZART-4 results and CMAQ results. For future O_3 changes for RCP4.5 in 2050 relative to 2000, the results between MOZART-4 and CMAQ are similar. Spatially, we compare the total co-benefits for both $PM_{2.5}$ and O_3 between MOZART-4 and CMAQ in Fig S20-S25. As for the 2000 comparison, we see that CMAQ better simulates air quality changes in urban environments at finer scale. So we add one sentence in Pg 9 Line 27 "CMAQ better simulates air quality changes in urban environments at a finer scale compared with MZ-4."

Figure S2 and Figure S3: the change in precipitation The values seem to be huge. The Figure S1 in the supporting information in Gao et al, 2014 (Gao et al., 2014, Robust spring drying in the southwestern U.S. and seasonal migration of wet/dry patterns in a warmer climate, Geophys. Res. Lett., 41, 1745–1751) shows the average annual precipitation of US is smaller than 5 mm/day (see the top left panel from that figure). Mean summer precipitation change was shown in Figure 20 in the following paper (Eric D. Maloney, et al., 2014: North American Climate in CMIP5 Experiments: Part III: Assessment of Twenty-First-Century Projections. J. Climate, 27, 2230–2270), and mean winter precipitation was shown in Figure 1 in the following paper (Neelin et al., 2013: California Winter Precipitation Change under Global Warming in the Coupled Model Intercomparison Project Phase 5 Ensemble. J. Climate, 26, 6238–6256). Both of these two papers show increase of precipitation at about 1-2 mm/day from CMIP5 multi-ensemble mean results. The statement on Page 5, Line 25, "US average increase of precipitation 8.16 and 7.63 mm/day" is a rather large value. Please double check the WRF simulations. In addition, please look at historical precipitation, and see if it is comparable to PRISM (top left panel of the figure below; PRISM is an observational dataset). This comment also applies to Fig. 1 in the main paper showing large differences in precipitation between RCP 4.5 and RCP 8.5. Thus, to have a reasonable historical 3-year precipitation spatial distribution is essential before these comparison s make sense. Page 5, Line 29-30: "However, the only region where the regional climate is warmer and drier in RCP 4.5 is in the Northwest US". This statement does not make too much sense. Because RCP 4.5 mostly show smaller warming than RCP 8.5, which is expected, thus, the statement regarding "drier" should not combine with the warming statement. In particular, I am afraid this feature from a single model WRF may not be robust.
Response: We thank the reviewer for calling our attention to this error and for providing detailed references. We became aware of this error soon after this paper was submitted, and found that it was due to our miscalculation of rainfall changes. We corrected this error in the new manuscript by replacing the plots in 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 decreases of 0.20 and 0.15 mm day^{-1} in RCP8.5 and RCP4.5." Also, as mentioned

in our first response, we compared WRF and GFDL AM3 annual average precipitation against CPC Unified Precipitation. We used the CPC precipitation instead of PRISM because of the horizontal resolution of CPC is of closer to WRF.

We deleted this text as suggested by the reviewer (Pg 5, Line 29-30):
"However, the only region where the regional climate is warmer and drier in RCP 4.5 is in the Northwest US".

Section 3.5: Co-benefit from domestic and foreign GHG mitigation. For domestic effect: S_Dom vs. S_REF (in Table 1). This is fine since the only differences between these two scenarios are emissions in US For foreign GHG mitigation: the authors compared S_RCP45 vs. S_Dom. I don't think these two scenarios can be interpreted as foreign GHG mitigation effect. Although the authors pointed out the limitations on Page 13 (Lines 11-15), I think the effect from US could be quite large. Since the authors did not conduct an experiment by reducing GHG over other countries only, the discussions related to foreign GHG mitigation should be revised (Figs. 7,8)
Response: We have constructed the scenarios to separate the total co-benefits (S_RCP45—S_REF) into components due to domestic emissions reductions (S_Dom—S_REF) and foreign emission reductions (S_RCP45—S_Dom). In comparing S_Dom and S_REF, the only difference is domestic air pollutant emissions. This provides a clean comparison to give an understanding of the importance of domestic emission changes, although it neglects the effect of US emissions on air quality through changes in global climate (via GHGs), regional climate (via short-lived climate pollutants), and methane. We consider that global climate and methane are mainly due to foreign emissions (as US emissions are much smaller than the rest of the world) and we neglect possible changes in regional climate from reductions in US short-lived climate pollutants.

We mainly focus on the effect of domestic reductions and compare that with the total. But we also find the effects of foreign reductions by simple subtraction (S_RCP45—S_REF) – (S_Dom—S_REF) = S_RCP45—S_Dom. In doing so, we attribute the effects of all climate changes and methane changes to foreign reductions (as most GHGs and methane are from foreign sources). Doing more work to better understand changes in regional climate due to emission mitigation, and to attribute those changes to US emissions, would require extra simulations with WRF, SMOKE, and CMAQ; as these are computationally intensive, they are beyond what we can perform for this study.

We have improved the text in the Methods sections to better clarify the logic of our simulation design, and to better communicate its limitations (Pg 8 line 3):
"By comparing S_Dom (applying GHG mitigation from RCP4.5 scenario in the U.S. only) with S_REF, and S_RCP45 with S_Dom, we quantify the co-benefits from domestic and foreign GHG mitigation. The co-benefits from foreign reductions are found by simple subtraction (S_RCP45 – S_REF) – (S_Dom – S_REF) = S_RCP45 – S_Dom. In estimating the co-benefits of domestic reductions, we account for the influences of methane and of global climate change as foreign influences (as most methane and GHG emissions are outside of the U.S.), and assume that U.S. air pollutant emissions have small effects on global or regional climate, such as through aerosol forcing."

There are quite a few places showing website link and accessed by a certain date. I suggest to move those links as footnotes and remove the words of “accessed date ***”.
Response: According to the ACP website:
http://www.atmospheric-chemistry-and-physics.net/for_authors/manuscript_preparation.html
Footnotes: *These should be avoided, as they tend to disrupt the flow of the text. If absolutely necessary, they should be numbered consecutively. Footnotes to tables should be marked by lowercase letters*.)
We have chosen to keep these as they are. If the paper is accepted for publication, we will consult with the editor on whether our format is proper.

Page 8, Line 24-25 The authors claimed that US EPA (2007) recommended use median instead of mean for evaluation. In fact, I did not find this statement. US EPA (2007) does recommend NMB, MNB, and a few other metrics MFB, MFE. They also showed benchmarks (Page 252-261), although there is a range of the benchmarks. The biases (in %) shown in Table 3 is relatively small. However, I am not quite sure the evaluation of median is a good way. Did the authors look at the mean in other way if the authors do not prefer to show metrics with mean value, i.e., spatial patterns?
Response: The right reference should be from Appel et al. (2008) instead of the EPA (2007) report. We corrected in the main paper. Appel et al. (2008) suggested to use “median” over “mean” when the species evaluated are not normally distributed, which is common for PM species. They also suggested that if the data were normally distributed, the mean and median would be the same (section 4 paragraph 5 in Appel et al., 2008).

Page 8, Line 30 “emissions are derived from global datasets rather than specific emissions for US” The authors mentioned in section 2.2 that they used SMOKE to process emissions. I don’t quite understand why the emissions are not specific for US.
Response: Here we mean that we are not using the NEI 2001 emission dataset, and instead downscale from the global RCP emissions in 2000. We rephrase this sentence to make it more clearly in Pg 8 Line 29-30:
“Model performance is not expected to be perfect as meteorology does not correspond with actual year 2000 meteorology, and emissions are derived from global datasets rather than the specific NEI dataset for the U.S.”

Page 9, Line 15 “NOx titration” should be “NO titration” Figure S14: S_RCP45 is largely due to NO titration. However, in Figure S13, RCP 8.5 scenario or REF scenario, increase in methane concentration should play a big role in ozone increases. In fact, line 9-18 of Page 9 refers to the figures in the supporting information, which may not be an ideal way to present.
Response: We changed from “NOx titration” to “NO titration”. See Pg 9, Line 15.
We agree that methane increases in RCP8.5 plays an important role in winter O_3 increases in the U.S., as suggested by the sensitivity simulation of Gao et al., (2013). We added new sentences in Pg 9, line 15-16:
“O_3 increases over the Northeast and West U.S. in winter in both S_REF and S_RCP45, caused by the weakened NO titration as a result of the large NO decrease in the two scenarios (Table 2), as also reported by other studies (Gao et al., 2013; Fiore et al., 2015), and as well as the large methane increases in RCP8.5 (Gao et al., 2013).”

With respect to the fact that Figures S12-S15 are in the supporting information, we have chosen to emphasize the co-benefits (Section 3.3) in this paper and so have included figures addressing the co-benefits in the main paper (Figs. 3-5). The changes relative to the year 2000 (Section 3.2) are included in the paper mainly as backdrop for understanding the future simulations, and for comparison against other studies that simulated 2050 relative to 2000 for these same scenarios. We appreciate the reviewer's suggestion to put one or more of these figures in the main paper, but have chosen to keep them in the supporting information so that the main paper will not be excessively long.

Page 10, line 25-26 and Table 1 for S_Emis and S_REF. These two scenarios (S_Emis and S_REF) were used to evaluate the impact from emissions. Since in standard RCP 8.5, methane concentrations show dramatic increase which is closely related to ozone concentrations. My question is whether CH_4 should keep the same as S_REF in S_Emis so as to be considered as RCP 8.5.

Response: By comparing between S_REF and S_Emis, we want to see the effect of global emission reductions from the RCP4.5 scenario, separate from changes in climate. These are the two mechanisms of co-benefits identified and quantified by West et al. (2013). We use methane concentrations from RCP4.5 in S_Emis to simulate the effect of all emission reductions, including both methane and short-lived air pollutants. The reviewer asks about RCP8.5, but we use the REF scenario for emissions throughout the paper and not RCP8.5. This design of scenarios (Table 1) is consistent with the global simulations of West et al., (2013). We have added text to the discussion of Table 1 to clarify that methane decreases in S_Emis (Page 8, Line 1-3): "The emission benefit from the first mechanism is calculated as the difference between S_Emis and S_REF, for which the change in methane concentration is included as an emission benefit, and the meteorology benefit is calculated as S_RCP45 minus S_Emis."

Fig. 6 (Page 29) should have titles for readers easy to recognize. The note of S_Emis-S_REF (described on Page 10, Line 26) is also useful to be added in the titles.

Response: We added the notes of "S_Emis-S_REF" and "S_RCP45-S_Emis" into Fig 6. We also added the notes in Fig 7.

Page 13, Line 8 "only one model is used at each step during downscaling". This sentence needs to be rephrased. For example, simply removing "at each step" may work better.

Response: We have revised text in Pg 13 line 8 as:

"Moreover, only one model is used during downscaling for regional climate (WRF) and air quality (CMAQ) modeling, and the mean of a model ensemble can be used to reduce model error."