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Interactive comment on "Co-benefits of global and regional greenhouse gas mitigation on U.S. air quality in 2050" *by* Yuqiang Zhang et al.

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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:

Major comments:

1. 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 me-

teorology 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.

2. 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 comparisons 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.

3. 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)

Minor comments:

1. 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 ***".

2. 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?

3. 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.

4. 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.

5. 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 CH4 should keep the same as S_REF in S_Emis so as to be considered as RCP 8.5.

6. 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.

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.

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