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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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General Comments

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.

Specific Comments

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.

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

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.

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.

Technical Corrections

Page 5, line 25: That unit can't be correct. ?? Figure 1b has the same issue. 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? Page 8, line 24-26: it seems you used median for both ozone and PM2.5? Can you justify? Page 9, line 6: These are switched, are they not? Over-prediction is higher for MDA8? Page 13, line 5-7: Seems there is an error in this sentence.

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