Response to comments #2

RC2 comments:
The work by Zhang et al. examines health impacts via air quality changes stemming from emissions changes in China from 2010-2017, expanding the role of $PM_{2.5}$ and O_3 and estimating the domestic vs international impacts. Overall the study is well posed. While other studies have examined this question specifically in China, here the authors focus on global-scale analysis, although in the end their findings support that a China-focused study would be sufficient, as >90% of the health impacts occur domestically. That aside, it's still likely sufficiently novel and interesting to ultimately warrant publication, however the paper itself needs some additional work in a few areas. These are described in detail in the comments below.
Response: We thank the reviewer's very positive comments of our study. We have revised the paper to take those comments into account. We provide detailed responses below (reviewers' comments in plain font, our replies in blue), and very much appreciate the reviewers' time.

Major comments:
Section 2.1: Please provide more details on which species are included in this model's estimate of $PM_{2.5}$. List primary and secondary species, both inorganic and organic. Describe how $PM_{2.5}$ itself is defined / calculated, i.e. is H_2O included, at what RH, and at what temperature and pressure are all values calculated. Also, what is the height of the top of the first model layer? Are O_3 concentrations adjusted from this height to the surface-level (typically 2m)?
Response: We thank the reviewer's suggestion. We now add the details for the $PM_{2.5}$ calculation in line 99-107:
"Secondary organic aerosols (SOA) are derived using the two-product model approach, with laboratory derived yields for monoterpenes, isoprene, and aromatic photooxidation (Heald et al., 2008; Times et al., 2016). Recent research has suggested that anthropogenic SOA may be a dominant contributor of health impacts globally (Nault et al., 2021). As our simulations lack representation of important anthropogenic SOA precursors, such as Intermediate-Volatility Organic Compounds (IVOCs; Zhao et al., 2014; Lu et al., 2020; Pennington et al., 2021), our simulated $PM_{2.5}$ concentrations may be low biased. $PM_{2.5}$ is calculated as the sum of $SO_4+NO_3+NH_4+OC+BC+SOA+0.2*Dust+Seasalt$ (West et al., 2013; Silva et al., 2016). For dust and sea salt, only the size fractions relevant for $PM_{2.5}$ (size bins 1-3) are used. Dust in desert regions was found to be too high in the model, so global dust concentrations were multiplied by 0.2 to achieve rough consistency with the $PM_{2.5}$ concentrations estimated with Brauer et al. (2012)."

Line 110:
"The lowest modeled gridcell (~58 m above the surface) is taken to indicate ground-level concentrations."

Rereence:

Brauer, M., Amann, M., Burnett, R. T., Cohen, A., Dentener, F., Ezzati, M., Henderson, S. B., Krzyzanowski, M., Martin, R. V, Van Dingenen, R., van Donkelaar, A. and Thurston, G. D.: Exposure assessment for estimation of the global burden of disease attributable to outdoor air pollution., Environ. Sci. Technol., 46(2), 652–60, doi:10.1021/es2025752, 2012.

Heald, C. L., Henze, D. K., Horowitz, L. W., Feddema, J., Lamar- que, J.-F., Guenther, A., Hess, P. G., Vitt, F., Seinfeld, J. H., Goldstein, A. H., and Fung, I.: Predicted change in global secondary organic aerosol concentrations in response to future climate, emissions, and land use change, J. Geophys. Res.-Atmos., 113, D05211, doi:10.1029/2007JD009092, 2008.

Silva, R. A., West, J. J., Lamarque, J.-F., Shindell, D. T., Collins, W. J., Dalsoren, S., Faluvegi, G., Folberth, G., Horowitz, L. W., Nagashima, T., Naik, V., Rumbold, S. T., Sudo, K., Takemura, T., Bergmann, D., Cameron-Smith, P., Cionni, I., Doherty, R. M., Eyring, V., Josse, B., MacKenzie, I. A., Plummer, D., Righi, M., Stevenson, D. S., Strode, S., Szopa, S., and Zengast, G.: The effect of future ambient air pollution on human premature mortality to 2100 using output from the ACCMIP model ensemble, Atmos. Chem. Phys., 16, 9847–9862, https://doi.org/10.5194/acp-16-9847-2016, 2016.

Tilmes, S., Lamarque, J. F., Emmons, L. K., Kinnison, D. E., Marsh, D., Garcia, R. R., Smith, A. K., Neely, R. R., Conley, A., Vitt, F., Val Martin, M., Tanimoto, H., Simpson, I., Blake, D. R. and Blake, N.: Representation of the Community Earth System Model (CESM1) CAM4-chem within the Chemistry-Climate Model Initiative (CCMI), Geosci. Model Dev., 9(5), 1853–1890, doi:10.5194/gmd-9-1853-2016, 2016.

West, J. J., Smith, S. J., Silva, R. A., Naik, V., Zhang, Y., Adelman, Z., Fry, M. M., Anenberg, S., Horowitz, L. W. and Lamarque, J. F.: Co-benefits of mitigating global greenhouse gas emissions for future air quality and human health, Nat. Clim. Chang., 3(10), 885–889, doi:10.1038/nclimate2009, 2013.

Somewhere this paper needs to address the significant issue of estimating $PM_{2.5}$ health impacts at such coarse spatial resolutions. Several previous studies, which are easy to find, have shown that biases can be up to 20-40% in estimates at these scales in global models.

Response: We thank the reviewer pointing this out. We now add the following sentences in line 157-163 to discuss the coarse resolution of the model we employed:

"Previous studies have shown that coarse resolution global CTMs, e.g. 1.9°×2.5°, likely generate low biases in estimating health effects, especially in urban areas (Li et al., 2016; Punger and West, 2013; Silva et al., 2013, 2016). However, less is known how these underestimates would affect the relative contributions of downwind transportation (Liang et al., 2018). Jin et al. (2019) concluded that the uncertainties in estimating the ambient $PM_{2.5}$-related mortality burden is dominated by the uncertainties in the underlying exposure-response function and less influenced by the uncertainties associated with the $PM_{2.5}$ concentration estimates."

Reference:

Jin, X., Fiore, A. M., Civerolo, K., Bi, J., Liu, Y., Van Donkelaar, A., Martin, R. V., Al-Hamdan, M., Zhang, Y., Insaf, T. Z., Kioumourtzoglou, M. A., He, M. Z. and Kinney, P. L.: Comparison of multiple $PM_{2.5}$ exposure products for estimating health benefits of emission controls over New York State, USA, Environ. Res. Lett., 14(8), 84023, doi:10.1088/1748-9326/ab2dcb, 2019.

Li, Y., Henze, D., Jack, D. and Kinney, P.: The influence of air qual- ity model resolution on health impact assessment for fine par- ticulate matter and its components, Air Qual. Atmos. Health, 9, 51–68, doi:10.1007/s11869-015-0321-z, 2016.

Liang, C.-K., West, J. J., Silva, R. A., Bian, H., Chin, M., Davila, Y., Dentener, F. J., Emmons, L., Flemming, J., Folberth, G., Henze, D., Im, U., Jonson, J. E., Keating, T. J., Kucsera, T., Lenzen, A., Lin, M., Lund, M. T., Pan, X., Park, R. J., Pierce, R. B., Sekiya, T., Sudo, K., and Takemura, T.: HTAP2 multi-model estimates of premature human mortality due to intercontinental transport of air pollution and emission sectors, Atmos. Chem. Phys., 18, 10497–10520, https://doi.org/10.5194/acp-18-10497-2018, 2018.

Punger, E. M. and West, J. J.: The effect of grid resolution on estimates of the burden of ozone and fine particulate matter on pre- mature mortality in the USA, Air Qual. Atmos. Health, 6, 563– 573, doi:10.1007/s11869-013-0197-8, 2013.

Silva, R. A., West, J. J., Zhang, Y., Anenberg, S. C., Lamarque, J.-F., Shindell, D. T., Collins, W. J., Dalsoren, S., Faluvegi, G., Folberth, G., Horowitz, L. W., Nagashima, T., Naik, V., Rumbold, S., Skeie, R., Sudo, K., Takemura, T., Bergmann, D., Cameron-Smith, P., Cionni, I., Doherty, R. M., Eyring, V., Josse, B., MacKenzie, I. A., Plummer, D., Righi, M., Stevenson, D. S., Strode, S., Szopa, S. and Zeng, G.: Global premature mortality due to anthropogenic outdoor air pollution and the contribution of past climate change, Environ. Res. Lett., 8(3), 034005, doi:10.1088/1748-9326/8/3/034005, 2013.

Silva, R. A., West, J. J., Lamarque, J.-F., Shindell, D. T., Collins, W. J., Dalsoren, S., Faluvegi, G., Folberth, G., Horowitz, L. W., Nagashima, T., Naik, V., Rumbold, S. T., Sudo, K., Takemura, T., Bergmann, D., Cameron-Smith, P., Cionni, I., Doherty, R. M., Eyring, V., Josse, B., MacKenzie, I. A., Plummer, D., Righi, M., Stevenson, D. S., Strode, S., Szopa, S., and Zengast, G.: The effect of future ambient air pollution on human premature mortality to 2100 using output from the ACCMIP model ensemble, Atmos. Chem. Phys., 16, 9847–9862, https://doi.org/10.5194/acp-16-9847-2016, 2016.

A recent study (Nault et al., ACP, 2021) showed that ~15% of $PM_{2.5}$ associated deaths may come from anthropogenically influenced SOA. This component of $PM_{2.5}$ would be highly sensitive to the emissions impacted by APPCAP. Was this accounted for in the simulated changes in total $PM_{2.5}$?

Response: That is an excellent question! The SOA module in the version of CAM_chem (CAM4, two-model product as described by Heald et al., 2008) employed in our study did not take into account of the explicit anthropogenic secondary organic aerosol (Lamarque et al., 2012; Tilmes et al., 2016). Based on the results of Nault et al. 2021, this omission could lead to underestimation of the $PM_{2.5}$-related mortality burdens at both urban and regional scales in our study. We add this discussion into line 100-102:

"The uncertainties in estimating accurate anthropogenically influenced SOA (Zheng et al., 2018; Cao et al., 2018) could make our model simulated $PM_{2.5}$ concentrations conservative (Nault et al., 2021)."

Reference:

Cao, H., Fu, T.-M., Zhang, L., Henze, D. K., Miller, C. C., Lerot, C., Abad, G. G., De Smedt, I., Zhang, Q., van Roozendael, M., Hendrick, F., Chance, K., Li, J., Zheng, J., and Zhao, Y.: Adjoint inversion of Chinese non-methane volatile organic compound emissions using space-based observations of formaldehyde and glyoxal, Atmos. Chem. Phys., 18, 15017–15046, https://doi.org/10.5194/acp-18-15017-2018, 2018.

Heald, C. L., Henze, D. K., Horowitz, L. W., Feddema, J., Lamar- que, J.-F., Guenther, A., Hess, P. G., Vitt, F., Seinfeld, J. H., Goldstein, A. H., and Fung, I.: Predicted change in global secondary organic aerosol concentrations in response to future climate, emissions, and land use change, J. Geophys. Res.-Atmos., 113, D05211, doi:10.1029/2007JD009092, 2008.

Lamarque, J.-F., Emmons, L. K., Hess, P. G., Kinnison, D. E., Tilmes, S., Vitt, F., Heald, C. L., Holland, E. a., Lauritzen, P. H., Neu, J., Orlando, J. J., Rasch, P. J. and Tyndall, G. K.: CAM-chem: description and evaluation of interactive atmospheric chemistry in the Community Earth System Model, Geosci. Model Dev., 5, 369–411, doi:10.5194/gmd-5-369-2012, 2012.

Nault, B. A., Jo, D. S., McDonald, B. C., Campuzano-Jost, P., Day, D. A., Hu, W., Schroder, J. C., Allan, J., Blake, D. R., Canagaratna, M. R., Coe, H., Coggon, M. M., DeCarlo, P. F., Diskin, G. S., Dunmore, R., Flocke, F., Fried, A., Gilman, J. B., Gkatzelis, G., Hamilton, J. F., Hanisco, T. F., Hayes, P. L., Henze, D. K., Hodzic, A., Hopkins, J., Hu, M., Huey, L. G., Jobson, B. T., Kuster, W. C., Lewis, A., Li, M., Liao, J., Nawaz, M. O., Pollack, I. B., Peischl, J., Rappenglück, B., Reeves, C. E., Richter, D., Roberts, J. M., Ryerson, T. B., Shao, M., Sommers, J. M., Walega, J., Warneke, C., Weibring, P., Wolfe, G. M., Young, D. E., Yuan, B., Zhang, Q., de Gouw, J. A., and Jimenez, J. L.: Secondary organic aerosols from anthropogenic volatile organic compounds contribute substantially to air pollution mortality, Atmos. Chem. Phys., 21, 11201–11224, https://doi.org/10.5194/acp-21-11201-2021, 2021.

Tilmes, S., Lamarque, J. F., Emmons, L. K., Kinnison, D. E., Marsh, D., Garcia, R. R., Smith, A. K., Neely, R. R., Conley, A., Vitt, F., Val Martin, M., Tanimoto, H., Simpson, I., Blake, D. R. and Blake, N.: Representation of the Community Earth System Model (CESM1) CAM4-chem within the Chemistry-Climate Model Initiative (CCMI), Geosci. Model Dev., 9(5), 1853–1890, doi:10.5194/gmd-9-1853-2016, 2016.

Other studies have shown that ammonium nitrate is a significant portion of $PM_{2.5}$ in this region. By not including nitrate, the simulated response of $PM_{2.5}$ to emissions will be rather muted. Can the authors estimate the magnitude of the uncertainty associated with this omission? I see they recognize this omission and others (lines 166-169), but it would be nice to see such approximations taken into account more quantitatively as part of their final results.

Response: We thank the reviewer for raising this issue. Lamarque et al. (2012) evaluated the performance of CAM-Chem in simulating the ammonium nitrate (CAM4, the same version used in our study) by comparing with surface observations from United States Interagency Monitoring of Protected Visual Environments (IMPROVE), and found that ammonium nitrate was fairly represented with a slightly smaller proportion for the high concentrations, possibly due to the model coarse resolution, and higher proportion for the low concentrations. However, we acknowledge that advanced treatment of nitrate in the chemical aerosol mechanism, as included in CAM5 (Tilmes et al., 2015), could potentially get accurate results with good reasons.

Reference:

Lamarque, J.-F., Emmons, L. K., Hess, P. G., Kinnison, D. E., Tilmes, S., Vitt, F., Heald, C. L., Holland, E. a., Lauritzen, P. H., Neu, J., Orlando, J. J., Rasch, P. J. and Tyndall, G. K.: CAM-chem: description and evaluation of interactive atmospheric chemistry in the Community Earth System Model, Geosci. Model Dev., 5, 369–411, doi:10.5194/gmd-5-369-2012, 2012.

Tilmes, S., Lamarque, J.-F., Emmons, L. K., Kinnison, D. E., Ma, P.-L., Liu, X., Ghan, S., Bardeen, C., Arnold, S., Deeter, M., Vitt, F., Ryerson, T., Elkins, J. W., Moore, F., Spackman, J. R., and Val Martin, M.: Description and evaluation of tropospheric chemistry and aerosols in the Community Earth System Model (CESM1.2), Geosci. Model Dev., 8, 1395–1426, https://doi.org/10.5194/gmd-8-1395-2015, 2015.

The abstract (and elsewhere) present premature mortality estimates with CI levels. These, I suspect, only reflect the uncertainty in the IERs. Given the substantial model errors and biases of up to 50%, how do these uncertainties compare to the uncertainties associated with model error? It would have been nice to see an attempt at incorporating the results of the model evaluation (section 3.1) into the subsequent analysis, rather than just touching on it in passing. They do touch on this on lines 269-275, but the text here is confusing. Why would the impact of bias be mitigated by high concentrations in China? Also, they point out here that the IER functions are non-linear. This is a reason why biases in the simulated $PM_{2.5}$ would make a difference, rather than be negligible, not this other way around.
Response: That is a good question. The IER function is super non-linear at low concentrations, but reaches linearity at higher concentrations, like in China (Burnett et al., 2014). To compare our results with other studies using higher resolution regional CTMs (Zhang et al., 2019), our study shows that from 2013 to 2017, there were 112,700 avoided deaths related to $PM_{2.5}$ reduction in China (Table 4), similar to values of 130,600 (95%I, 115,900—159,200, Table S5) estimated by Zhang et al., 2019, Our previous study has shown that in estimating ambient $PM_{2.5}$-related mortality burden, the uncertainties from concentration-response functions (such as IER) derived from epidemiology are much higher than the uncertainties from the $PM_{2.5}$ concentration estimations (Jin et al., 2019).

Reference:

Jin, X., Fiore, A. M., Civerolo, K., Bi, J., Liu, Y., Van Donkelaar, A., Martin, R. V., Al-Hamdan, M., Zhang, Y., Insaf, T. Z., Kioumourtzoglou, M. A., He, M. Z. and Kinney, P. L.: Comparison of multiple $PM_{2.5}$ exposure products for estimating health benefits of emission controls over New York State, USA, Environ. Res. Lett., 14(8), 84023, doi:10.1088/1748-9326/ab2dcb, 2019.

Zhang, Q., Zheng, Y., Tong, D., Shao, M., Wang, S., Zhang, Y., Xu, X., Wang, J., He, H., Liu, W., Ding, Y., Lei, Y., Li, J., Wang, Z., Zhang, X., Wang, Y., Cheng, J., Liu, Y., Shi, Q., Yan, L., Geng, G., Hong, C., Li, M., Liu, F., Zheng, B., Cao, J., Ding, A., Gao, J., Fu, Q., Huo, J., Liu, B., Liu, Z., Yang, F., He, K. and Hao, J.: Drivers of improved $PM_{2.5}$ air quality in China from 2013 to 2017, Proc. Natl. Acad. Sci. U. S. A., 116(49), 24463–24469, doi:10.1073/pnas.1907956116, 2019.

Table 2 and 3 are useful. It would also be nice to see maps of the station measurements overlaid on top of model estimated surfaces. If biases / errors are particularly large in regions most impacting export (NE), that would be useful to know, given that the main stated novelty of this work (line 87) is examining the impact of these changes on global air quality, not just domestically.
Response: Thanks for the suggestion. We now add two new plots S3-S4 in the supporting material showing the normalized mean biases (NMBs) overlaid on top of model estimated concentration (5-yr average annual $PM_{2.5}$ and MDA8 ozone from 2013 to 2017). From the plots, we see that for both $PM_{2.5}$ and ozone, the NMBs for the 5-yr (2013-2017) are both lower in the eastern coast, compared with other regions in China. We add a new sentence in line 190-191: "For both $PM_{2.5}$ and ozone, we also find that the NMBs are lower in the eastern China compared with other inland regions (Figs. S5-S6)."

Fig. S5. Evaluation of simulated annual $PM_{2.5}$ concentration against surface observations. The circles depict 249 locations of continued valid $PM_{2.5}$ observations from 2013 to 2017 (normalized mean bias(NMB), horizontal colorbar), overlaying on the 5-yr average of model simulated annual concentration (μg m^{-3}, vertical colorbar).

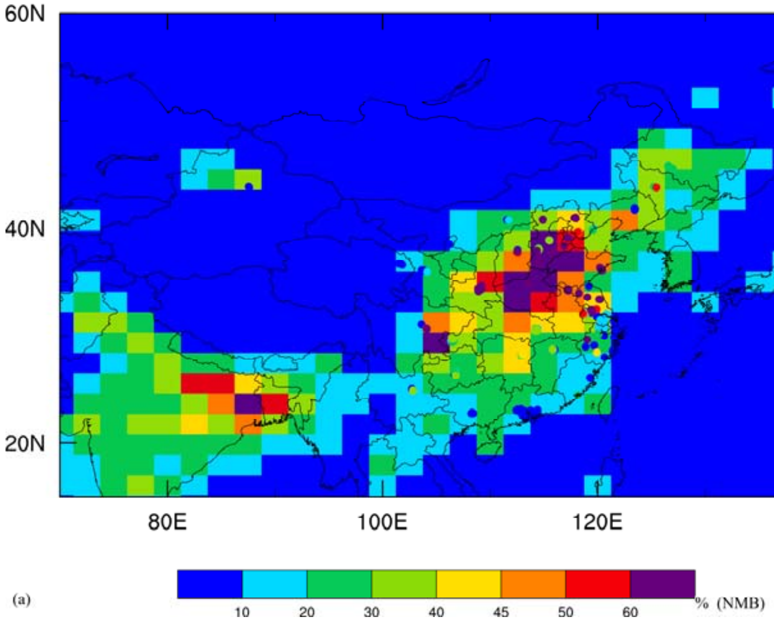
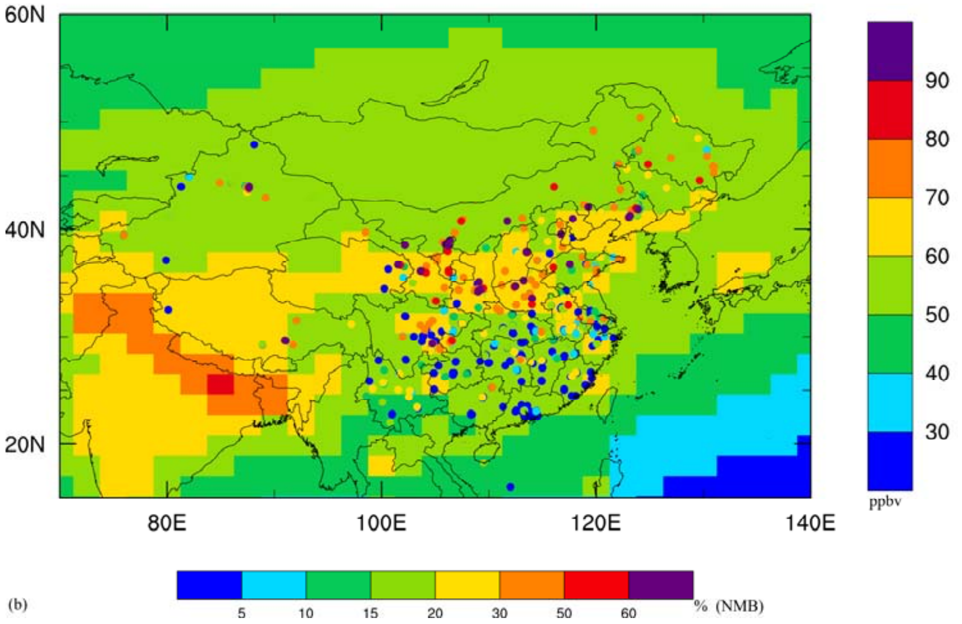


Fig. S6. Evaluation of simulated annual MDA8 ozone concentration against surface observations. The circles depict 700 locations of continued valid ozone observations from 2013 to 2017 (normalized mean bias(NMB), horizontal colorbar), overlaying on the 5-yr average of model simulated concentration ppbv, vertical colorbar).



The discussion is a bit hard to parse — it seems like the 2nd paragraph contains a lot of ideas, some which aren't well explained, and all of it mashed together in one final push of text that mixes sources of uncertainty with explanation of results and highlights of their findings. I'd recommend spending more time on this section.
Response: We thank the reviewer pointing this out. We now move the discussions of the uncertainties into 1st paragraph. In 2nd paragraph, we focus on the major findings and highlights from our study. See the updated manuscript below.

According to Fig 6, the $PM_{2.5}$ concentration impacts are significantly larger in South Korea than Japan and the US by 2017. However, in Table 4, for 2017 the mortality impacts are much bigger for Japan, and similar for South Korea and the US. Please check to make sure there wasn't a mixup. If not, please explain how this is the case (possibly given different mortality rates and populations). I see they note this point on line 282 but offer zero explanation.
Response: We appreciate the question. The higher mortality impact in Japan compared with South Korea is mainly driven by the population, with population in Japan ~2.5 times of the population in South Korea. We now add the explanation for this in line 243-246:
"Japan has a smaller change for the annual Pop-weighted $PM_{2.5}$, but much larger changes in $PM_{2.5}$-related mortality burden changes, ranging from 197 added premature deaths in 2011, and 875 avoided premature deaths in 2017, mainly caused by the much higher population in Japan than that in South Korea (https://countryeconomy.com/countries/compare/japan/south-korea?sc=XE23, last accessed Sep 3rd, 2021). "

Writing: the paper is in pretty rough shape. I doubt that many of the co-authors have contributed in detail or paid much attention to the final draft, given the prevalence of basic issues. I started making a note of corrections but tired of this after the first ten lines. Please proof-read and polish the writing throughout, it was quite distracting and at times confusing.
Response: We thank the reviewer's comment. We now ask our coauthors (Dr. Karl Seltzer) who are native speakers to polish our draft.

For example:
19: of —> of the
Response: change as suggested.

20: As a statistical aspect, it seems like human health benefits are hard to observe. Maybe rephrase?
Response: We now rephrase this sentence as the following:
"These changes have resulted in significant air quality improvements that are reflected in both surface networks and satellite observations."

20: $PM_{2.5}$ and surface —> $PM_{2.5}$, surface
Response: change as suggested

21: enough lifetime —> lifetimes
Response: change as suggested

21: which can —> to
Response: change as suggested

21: So emission —> Emission
Response: change as suggested

22: will —> will thus
Response: change as suggested

22: region air quality domestically —> domestic air quality
Response: change as suggested

22: but also —> but will also
Response: change as suggested

24: from the emission change —> from emissions changes
Response: change as suggested

24: the health —> health
Response: change as suggested

Minor comments:
Abstract: state which model is used for this study?
Response: change as suggested. See line 23-25:
"In this study, we use a global chemistry transport model (CAM-chem) to simulate the influence of Chinese emission changes from 2010 to 2017 on both domestic and foreign air quality."

32: Not sure what is meant by "at least" in this context. Why is this a lower bound?

Response: We use “at least” here to refer to the fact that for other years, more than 93% of $PM_{2.5}$-related mortality burdens changes happen in China, such as 94% in 2012, and 95% in 2013. We now rephrase this sentence:
“Relative to 2010, emission changes in China increased the global $PM_{2.5}$-associated premature mortality burdens through 2013, among which a majority of the changes (~93%) occurred in China. The sharp emission decreases after 2013 generated significant benefits for human-health. By 2017, emission changes in China reduced premature deaths associated with $PM_{2.5}$ by 108, 800 (92,800—124,800) deaths yr^{-1} globally, relative to 2010, among which 92% were realized in China.”

63: Are these numbers in response to the (arbitrary) 20% reduction in emissions used in the TF HTAP modeling tests?
Response: The reviewer is right that these numbers corresponded to the arbitrary 20% reduction employed by the TF-HTAP studies. We now make it clear in this sentence lines 56-62:
“Liang et al. (2018) used the ensemble model outputs from the Task Force on Hemispheric Transport of Air Pollution (TF HTAP, Janssens-Maenhout et al., 2015) and estimated the source-receptor relationship between air quality and avoided premature mortality from a 20% reduction in anthropogenic emissions in East Asia. They estimated that 96,600 premature mortalities from long-term $PM_{2.5}$ exposure could be avoided globally due to these emission reductions, with 6% (5,500 deaths) occurring in downwind regions. For long-term O_3 exposure, these emission reductions could lead to 1,400 fewer premature mortalities globally, with 15% (1,700 deaths) occurring downwind. ”

147-149: GBD methods evolve annually, so I wouldn’t say “latest” here.
Response: We now rephrase this sentence:
“retrieved from the Global Burden of Disease 2017 (GBD2017) study (Stanaway et al., 2018)”
Also in line 153:
“Country-age-specific baseline mortality rates (Y_0) in 2010 were retrieved from the GBD2017 project”

Section 2.3: Please also report: what counterfactual values were used, if any, for $PM_{2.5}$ and O_3? What metric was used for the O_3 concentration (annual average? 6 month? 1 hr or 8 hr max? etc).
Response: We thank the reviewer pointing this out. We rephrase the sentence in line 151-152:
“The RR for long-term O_3 exposure is retrieved from Turner et al., (2016), with reports a RR of 1.12 (95 % confidence interval (CI): 1.08, 1.16) for respiratory disease.”

We also add a new sentence in line 155-157:
“The theoretical minimum risk exposure level for $PM_{2.5}$ exposure assessment is drawn from a uniform distribution with a lower bound of 5.8 μg m^{-3} and an upper bound of 8.8 μg m^{-3}, and for O_3 exposure it is 26.7 ppbv.”

194: What drives the isolated increase in $PM_{2.5}$ in NW China shown in Fig 2?
Response: We suspect that the $PM_{2.5}$ increases in NW China was dominated by the dust storm (Meng et al., 2019; Luo et al., 2020; Zhao et al., 2020).
We now add the explanation in line 208-209:

“There were also isolated increases in $PM_{2.5}$ in northwest China from 2010 to 2013, which were mainly caused by the dust storms (Meng et al., 2019; Luo et al., 2020; Zhao et al., 2020)”

Reference:

Meng, L., Yang, X., Zhao, T., He, Q., Lu, H., Mamtimin, A., Huo, W., Yang, F. and Liu, C.: Modeling study on three-dimensional distribution of dust aerosols during a dust storm over the Tarim Basin, Northwest China, Atmos. Res., 218(December 2018), 285–295, doi:10.1016/j.atmosres.2018.12.006, 2019.

Luo, H., Guan, Q., Pan, N., Wang, Q., Li, H., Lin, J., Tan, Z. and Shao, W.: Using composite fingerprints to quantify the potential dust source contributions in northwest China, Sci. Total Environ., 742, 140560, doi:10.1016/j.scitotenv.2020.140560, 2020.

Zhao, J., Ma, X., Wu, S. and Sha, T.: Dust emission and transport in Northwest China: WRF-Chem simulation and comparisons with multi-sensor observations, Atmos. Res., 241(March), 104978, doi:10.1016/j.atmosres.2020.104978, 2020.

244: This was confusing, until I figured out they are referring specifically to 2011. Please confirm/ clarify.

Response: We rephrase this sentence:

“The emission changes in China increased the global ozone-related mortality by 4,900 (95%CI, 3,700—5,900) premature deaths yr^{-1} in 2011 (Table 5), among which 73% occurs in China (3600 premature deaths yr^{-1}, 95%CI: 2,700—4,300).”

247: This statement doesn’t make sense. How could the changes in China alone be 43% higher than the global total change? Do they mean just the international change (excluding China) for the latter?

Response: The increased global ozone-related mortality burden (5,920 deaths yr^{-1} in 2017) was lower than that in China (8,500 deaths yr^{-1}) was since the emission decreases in China in 2017 decreased the ozone concentration in downwind regions while increased ozone concentration in China.