Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2021-385-RC2, 2021

Comment on acp-2021-385

Anonymous Referee #2

Referee comment on "Impacts of emission changes in China from 2010 to 2017 on domestic and intercontinental air quality and health effect" by Yuqiang Zhang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-385-RC2, 2021

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 PM2.5 and O3 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.

Major comments:

Section 2.1: Please provide more details on which species are included in this model's estimate of PM2.5. List primary and secondary species, both inorganic and organic. Describe how PM2.5 itself is defined / calculated, i.e. is H2O 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 O3 concentrations adjusted from this height to the surface-level (typically 2m)?

Somewhere this paper needs to address the significant issue of estimating PM2.5 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.

A recent study (Nault et al., ACP, 2021) showed that ~15% of PM2.5 associated deaths may come from anthropogenically influenced SOA. This component of PM2.5 would be highly sensitive to the emissions impacted by APPCAP. Was this accounted for in the simulated changes in total PM2.5?

Other studies have shown that ammonium nitrate is a significant portion of PM2.5 in this region. By not including nitrate, the simulated response of PM2.5 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 prat of their final results.

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 tough 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 PM2.5 would make a difference, rather than be negligible, not this other way around.

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. Also it would be good to evaluate the biases by season, since export is stronger in the spring.

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.

According to Fig 6, the PM2.5 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.

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. For example:

19: of —> of the

20: As a statistical aspect, it seems like human health benefits are hard to observe. Maybe rephrase?

20: PM2.5 and surface —> PM2.5, surface

21: enough lifetime —> lifetimes

21: which can —> to

21: So emission —> Emission

22: will —> will thus

22: region air quality domestically —> domestic air quality

22: but also —> but will also

24: from the emission change —> from emissions changes

24: the health —> health

Minor comments:

Abstract: state which model is used for this study?

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

63: Are these numbers in response to the (arbitrary) 20% reduction in emissions used in the TF HTAP modeling tests?

147-149: GBD methods evolve annually, so I wouldn't say "latest" here.

Section 2.3: Please also report: what counterfactual values were used, if any, for PM2.5 and O3? What metric was used for the O3 concentration (annual average? 6 month? 1 hr or 8 hr max? etc).

194: What drives the isolated increase in PM2.5 in NW China shown in Fig 2?

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

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?