Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-779-RC1, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sectorial and regional uncertainty analysis of the contribution of anthropogenic emissions to regional and global $PM_{2.5}$ health impacts" *by* Monica Crippa et al.

Anonymous Referee #1

Received and published: 26 December 2017

The manuscript by Crippa et al. investigates the regional and sectoral contributions to PM2.5 and associated health impacts throughout the world. This is accomplished through application of the TM5-FASST response tool. This topic is useful and their results are new, and also appropriate for the scope of this journal. They also provide a much needed estimation of how uncertainty in the emissions estimates propagate into uncertainties in PM2.5 estimates, which is a source of error not often well quantified in health impact studies. That being said, the manuscript good use more attention to previous works, especially in the introduction. These and some additional comments are highlighted below, which include requests for more information about the fidelity of the modeling estimates used here, and the impact of a few assumptions in its applica-

Printer-friendly version

Discussion paper

tion that are made but not evaluated either through their own work presented here or references to literature (i.e. assuming PM2.5 responds linearly to emissions changes, or that anthropogenic SOA is negligible). Addressing these concerns constitutes major revisions, after which point this manuscript will be suitable for publication in ACP.

Major:

1.35: I wonder if the authors considered including some more recent estimates e.g. from the Global Burden of Disease project on estimated numbers of premature mortalities from ambient PM2.5 exposure, such as Cohen et al., The Lancet, 2017.

Ok, I see that relevant works be e.g. Lelieveld (2015), Silva (2016) or Cohen (2017) are finally discussed on page 11. Such works however should be discussed as part of the introduction and background information, in order to more clearly articulate the role of the present work.

In general the introduction was laking in some detail with regards to previous works that have considered sector-specific health impacts, the role of model uncertainty vs emissions uncertainties or uncertainties in concentration-response parameterizations on estimates of PM2.5 health impacts.

2.1: Suggest adding references to any number of studies that have estimated the human health impacts of sector-specific policies for PM2.5 reduction.

Equation 1: This equation is an approximation, not an equals sign. This should be clearly indicated, and the error associated with ignoring second-order terms should be discussed, either using evidence from the own authors work or from reference to many previous studies in the literature that have explored the nonlinear response of PM2.5 to emissions perturbations.

Section 2.2: Some essential details of the TM5-FASST model are missing. What is the accuracy of the baseline PM2.5 (total, and speciated) concentrations estimated by TM5 fast compared to in situ measurements in different parts of the world? In

locations where such data is not available, how do the model estimates compare to those from other models, or from remote-sensing derived products? How much error is expected owing to the coarse model resolution when estimating population-weighted concentrations, given the relatively high-resolution variability in population densities?

Ok - while I do see that there is a single paragraph addressing this in very broad strokes, on page 7 (21-28), this evaluation is incommensurate with the scales of the paper. Given the regional, sectoral and species specificity of the source attribution results, the authors need to examine model fidelity on the same scales.

11.31: Cohen et al. (2017) also report a range for the total estimated global premature deaths from ambient PM2.5 - which should be repeated here. This is interesting to consider, as the source of the uncertainty in the Cohen paper is from uncertainty in the concentration-response relationships (IERs), not from uncertainties in the exposure estimates that may be owing to uncertainties (in part) from emissions. However, the range of values cited here (+/- 1.1 million) indicates that this uncertainty associated with emissions estimates is an factor, which hasn't been much considered previously. This is an import results of the present work which I believe could be highlighted more (i.e. by comparing the magnitude of the emissions-driven uncertainties to the magnitude of other types of uncertainties considered in different studies). Quantitative summary of this (similar to the final sentence of the manuscript) would be nice to see in the abstract as well.

Minor:

2.10-2.14: What fraction of secondary PM2.5 long-range transport is owing to transport of the gas-phase precursors vs the transport of the secondarily formed PM2.5 itself?

2.27: Clarify here that this inventory, and the prescribed emissions for these experiments, pertain only to anthropogenic emissions.

3.14: Can the authors comment on the validity of this assumption, as backed up by

Printer-friendly version

Discussion paper

their own investigations or those in previous studies in the literature?

3.23: The source-receptor modeling was based around a single year that didn't alight with the year of the emissions considered. To what extent does this misalignment potentially impact results? Or to what extent is the meteorology in this particular year representative of a climatological average? I guess I'm just wondering if the authors have checked if 2001 was for any reason particularly extreme with regards to temperature, precipitation, transport, or sources of natural PM2.5 such as biomass burning?

3.27: To what extent does not including anthropogenic SOA influence conclusions about the role of different sectors?

4.4: It seems that rather than aggregation the authors could consider some metrics that are normalized with regards to the country size or population.

4.20: Here and elsewhere the Janssens-Maenhout (2017, submitted) paper is cited, although it's hard to evaluate what information is contained therein about

Figure 1: It's not clear – are the % contributions to the average PM2.5 in each region, or to the population-weighted average PM2.5 in each region?

7.34: I think the impacts of the residential sector on indoor air quality are well known and have been documented in many previous studies, that could be cited.

7.39: Similarly, the role of the agricultural sector or NH3 in particularly has been noted in several previous and recent studies. The authors continue to cite only Maas and Grennfelt, 2016, despite the broader literature available for comparison.

8.9-11: Can the authors explain why primary emissions play such a large role in the uncertainty analysis, compared to their contribution to absolute PM2.5 concentration?

9.20: Given that this work doesn't include anthropogenic SOA, what is the role of NMVOCs in PM2.5 formation? I guess I was just surprised to see these mentioned here.

11.34: What is the “urban increment subgrid adjustment”?

11.33 - 35: I strongly agree that these factors are critical towards making these comparisons, as are sources of information such as populations densities and baseline mortality rates. For those precise reasons, the authors should provide details on these aspects as used in their study, as have been provided in the cited works, in order to make such comparisons possible and meaningful.

12.10-12: What it is about these regions that given them such relatively large era-regional contributions to PM2.5 health impacts?

Editorial:

2.23: “not to the least” change to “not the least”

2.35: "at sector" change to "at the sector"

2.36: "on the potential" change to "of the potential"

3.19: Some of this sentence seems to be missing.

4.16: "as following" change to "as follows"

6.16: "across" change to "an across"

8.23: "Europe the" change to "Europe, the"

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-779, 2017.

Printer-friendly version

Discussion paper