

## ***Interactive comment on “Long-term trends in the PM<sub>2.5</sub>- and O<sub>3</sub>-related mortality burdens in the United States under emission reductions from 1990 to 2010” by Yuqiang Zhang et al.***

### **Anonymous Referee #2**

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The manuscript by Zhang et al. examines trends in premature mortalities associated with exposure to ambient PM<sub>2.5</sub> and O<sub>3</sub> in the US over 3 decades. The work is valuable towards understanding the different factors driving these trends and in contributing to a body of evidence documenting the public health benefits of air pollution controls. I have some questions and suggestions that relate to how the authors present uncertainty in their analysis, how they distinguish their work from previous studies, and how they present the trends in different components of the premature mortality calculations (i.e. concentrations vs baseline mortality rates). Addressing these will amount to minor revisions, after which I believe this manuscript will be suitable for publication in ACP.

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Major comments:

1.26 and throughout: These confidence intervals only account for a subset of the uncertainties inherent in these estimates (i.e., they ignore any inaccuracies in the air pollution model). Thus, it should be clearly stated up front what these ranges do and do not represent. The same comment applies to other places where these numbers are prominently presented, such as e.g. Table 1.

2.25 - 3.6: I'm not sure I appreciate the significance of the differences between the work in this manuscript and the previous works of Cohen et al. (2017) and Fann et al. (2017), who estimated trends in premature mortalities associated with PM<sub>2.5</sub> (and O<sub>3</sub> – Cohen) in the US since 1980. Yes, their analysis was only once every 5 years, not every year, but does that really make a big difference in the overall conclusions? I'm not sure what the importance of studying successive years is, or interannual variability. The authors results shown in Fig 3 would seem to indicate the answer is “not much”, at least for PM<sub>2.5</sub>. I further think works such as Fann 2017 also do discuss different drivers of the trends (mortality rates, population. . .). So, I suggest the authors could go into more detail here about what these previous studies found, including their quantitative results, and also how the present work goes beyond these previous studies methodologically. Update: upon reading section 3.3 (Comparison to previous studies), I'm more informed about how these results differ. Yet still, it would probably benefit the authors to put some more of this content up front in the introduction for motivation. At the very least, Section 3.3 could be alluded to in outlining the contents of what is to come (3.7-12).

General: I have some confusion about how to separately interpret the impacts of changing concentrations from changing mortality rates. At present the manuscript seems to imply that changes in baseline mortality rates are not benefits of improving air pollution. However, if reductions in concentrations improve air quality, wouldn't this lead to reductions in mortality rates? To what extent are the evolution of these two terms in the health impact function separate? Could the authors comment on an explain this a bit more?

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4.23: What was the basis for using an average value for the threshold? What impacts does this value have on the overall findings, quantitatively and qualitatively?

4.27: Again, what is the basis for this, and how does it affect the findings?

Results: Please provide as separate figures (there is plenty of room for this within the main body of an ACP paper): (1) trends in baseline mortality rates, (2) trends in concentrations, (3) trends in AF, (4) trends in mortalities. Providing these pieces of information separately would make parsing the results of this paper so much more straightforward. At less than 10 pages of text and only 6 figures, presently, there's no reason to place any of this in the SI.

Fig 5: I strongly object to the choice of color scale for panel (a). The potential for misunderstanding the results is quite high. Please use red-blue colors scales as commonly understood, which blue being negative and red being positive. Or pick an entirely different color scheme for these results that are strictly negative.

Minor comments and corrections:

18: Perhaps, more precisely, "exposure to these pollutants are associated with..."

abstract: From the perspective of air quality control and the audience of ACP, it seems more interesting to report how many premature mortalities would have been avoided by PM<sub>2.5</sub> and O<sub>3</sub> reductions in the absence of changes in baseline mortality rates (rather than the other way around). That being said, perhaps these are the numbers that are reported in the last few lines of the abstract? It's not clear if these are / are not accounting for changes in mortality rates (or population).

2.15: observations sites → observations

2.20: it's note entirely clear how the authors are separating the benefits of the NAAQS from those of the technologies put in place to meet the NAAQS – seems like these are perhaps two sides of the same coin.

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2.24: I understand the wording, but if one accept the health impact analysis framework used here, then does it matters not where people live as much as where they die?

2.15 - 2.24: Note the difference in tone between the assertiveness of this work (“improvements were mainly driven by ambient air quality standards...”, and that of Fann 2017 who state “it is difficult to attribute this reduction to specific policy interventions...many factors are likely to have contributed...federal air quality policies are likely to have played an important role”. It then seems that different federal regulations are cited, such as Acid Rain program. Thoughts about why the present work is a bit more sure of the role of regulations in this regard? Were EPA authors just being more cautious with their wording for professional reasons?

4.16: I doubt this is what the authors meant to say. If RRs were downloaded from the GBD web site, then there would be no role for the simulations of O<sub>3</sub> and PM<sub>2.5</sub> concentrations described in section 2.1.

4.25: The justification here seems a bit odd, as if the authors decided that sticking with the mismatched population age-ranges from their previous work would take precedent over correctly matching age ranges with the epidemiological study of Jerrett 2009.

4.28 - 5.15: Would it be possible to avoid these types of inconsistencies in reporting to use health impact functions associated with all-cause rather than cause-specific mortality rates, even if the epidemiological evidence of the responses isn't as robust?

7.24: in the eastern

Title: should indicate this is about ambient AQ? Or that is obvious?

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