Responses to CASAC Questions on the PM PA from Consultant Dr. Dan Jaffe

In general: Many of these questions refer to the specifics of C-R models. Unfortunately this is outside of my expertise. I will try to address as many of your questions as possible. Thank you for the opportunity to assist this round of the NAAQS review.

Questions from Dr. Tony Cox

Question 1

I do not see a definition for the beta coefficients in the document and so do suggest that these need further clarification.

Question 2

The BenMAP-CE model is outside of my expertise.

Question 3

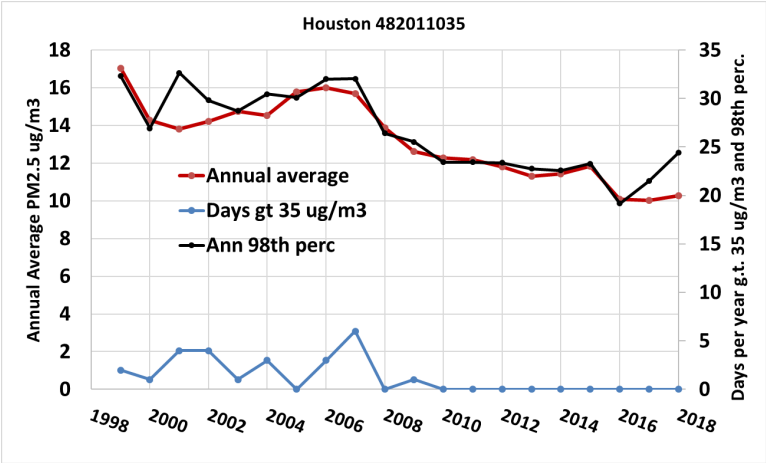
Concentration-response (C-R) is a broad term that could refer to many different aspects of the analysis. I think it is fair to use here in a general sense, but specific applications need clarity so as the meaning is clear in each case.

Other questions are outside my area of expertise.

Questions from Dr. Sabine Lange

Question 1

I looked over section 3.2.3.2.1 and especially Figure 3-5. I agree that the results and figure 3-5 are a bit puzzling. It would seem that these studies looking at short term exposures should be compared against the daily $PM_{2.5}$ standard. In general there is not a good relationship between the annual average $PM_{2.5}$ and the number of days over the daily standard (35 ug/m^3). I also examined data from the Houston site (AQS id 482011035). Figure 1 below shows the annual average, annual 98th percentile and number of days over 35 ug/m^3 since 1999 and you can see there is not much relationship with the number of high days and the 98th percentile can be also disconnected with the annual average. For example in the last two years, there has been a significant increase in the 98th percentile, but not the annual average. Also, see Figure B-8 in PA document.



Question 2

I agree that the short term studies are most relevant to the daily $PM_{2.5}$ standard. For example one can imagine a case where two studies of the same city but performed at different times of the year could come to rather different conclusions. Eg. Most regions have higher $PM_{2.5}$ in winter, so a study in winter might shower greater health effects compared to a study down in summer and yet both would have identical annual design values. Based on the discussion in section 3.2.3.2.2 it does not seem like this has been considered.

Question 3

Yes, these studies assign all individuals in a geographic region into the same exposure category but I am not sure what is a better approach. Different communities and neighborhoods within a community have many differences, just as individuals have differences. The association between air pollution and health will depend on the $PM_{2.5}$ concentrations, but clearly there are complicating and confounding variables (smoking, occupation, age, sex, etc). In general most studies are able to identify and account for the confounding variables. To the extent that a key confounder is missed, this can invalidate the results.

The remaining questions are outside my area of expertise.

Questions from Dr. Mark Frampton

Question 1

The evidence for health impairment and premature mortality from $PM_{2.5}$ is extensive and fall into multiple categories. This includes epidemiological studies, mechanistic (biochemical) studies, clinical studies and animal studies. While the individual results from any one study could be questioned, the bulk of scientific papers provide strong evidence for health effects and premature mortality due to $PM_{2.5}$. The 2019 Draft Policy Assessment for $PM_{2.5}$ summarizes many of the scientific papers supporting this view.

Question 2

This question is very similar to that posed by Dr. Lange. Please see my answer above. All individuals are different. The question of heterogeneity across cities comes down to whether a key confounding variable has been missed. As a simple example, lets suppose that water quality in one region impacts health in a similar manner as $PM_{2.5}$. Clearly this could confound any results from the two cities. This is why the search for confounding variables is so important in epidemiology. To the best of my knowledge the major confounding variables have largely been addressed in air pollution epidemiology, but it is always possible that a new confounding variable could be identified.

Question 3

The preamble to the 2015 ISA seems like a valuable framework for judging both individual studies and the overall health impacts. While publication in peer-review journals is one important quality control step, it is not sufficient by itself to assess study quality. Section 4 of the ISA pre-amble seems especially important for judging individual studies and Table 1 suggests a useful framework for identifying causality. Given this framework, it would make sense to me that certain non-scientific analyses be excluded from the ISA. As one example, the outside consultants were provided a list of possible references to consider, and while I have not had time to examine them all, I would suggest that references such as this one, that we were provided, would not reasonably be included in the ISA or PA: https://doi.org/10.1016/j.gloepi.2019.100003.

The remaining questions are outside my area of expertise.

Questions from Dr. James Boylan

The discussion on primary sources seems accurate. The discussions on secondary aerosol is very cursory. This could be significantly improved.

On monitoring, yes this seems accurate. On trends there is too little discussion on the 98th percentile. We now that, at least for the western US, wildfires are increasing and having a significant influence on the 98th percentiles. See:

1. Abatzoglou, J. T. & Williams, A. P. Impact of anthropogenic climate change on wildfire across western US forests. *Proc. Natl. Acad. Sci.* 113, 11770–11775 (2016).
2. Dennison, P.E., Brewer, S.C., Arnold, J.D., Moritz, M.A., 2014. Large wildfire trends in the western United States, 1984–2011. Geophys. Res. Lett. 41, 2928–2933. https://doi.org/10.1002/2014GL059576
3. McClure C.D. and Jaffe D.A. US particulate matter air quality improves except in wildfire-prone areas. Proc.Natl.Acad.Sci., DOI: 10.1073/pnas.1804353115, 2018.
4. Westerling, A. L. Increasing western US forest wildfire activity: sensitivity to changes in the timing of spring. Phil. Trans. R. Soc. B 371: 1696 (2016).

On measurement biases: I am not aware of any studies showing consistently higher measured concentrations from FEMs compared to FRMs. (do you have a reference for this?)
I see this online from Minnesota Pollution Control Agency:
https://www3.epa.gov/ttn/amtic/files/2014conference/wedqamcmahon.pdf

there are many others. But none seem to suggest a persistent bias one way or the other.
Both FRM and FEM have potential weaknesses, particularly related to temperature and aerosol volatility. So where does this lead us? Maybe a better question is what is our uncertainty from both FRM and FEM measurements and are these uncertainties different? Based on the many comparisons that have been done, I suggest the uncertainties in $PM_{2.5}$ with either an FRM or FEM or of order 10%. These uncertainties are of similar magnitude as the uncertainties in spatial modeling.

Regarding background concentrations, I believe the discussion on background $PM_{2.5}$ is all reasonable, however I suggest more consideration be given to the implication of extreme events (international transport, wildfires, volcanoes, dust storms, etc) and their treatment in both health studies and exceptional events). While these infrequent events have little impact on the annual average, they can be especially important when considering the 98th percentile.

As for additional research:

I believe the spatial representation is a major uncertainty. It is very difficult to understand the exposure in densely populated urban areas that have few AQS monitoring sites. This would be especially true for neighborhoods in close proximity to freeways, other major roads and/or other sources. While the satellite fusion and hybrid modeling methods are all positive developments, I think even greater progress can be made by incorporation of low-cost sensor networks into fusion approaches. These are now being deployed around the country and world. There is a need for research to develop methods to utilize these data in some creative fusion approaches. Obviously sensor calibration and referencing to FRM and FEM observations is critical, but I believe this problem can be overcome. Other areas for research that I believe are important are identification of biological agents in the air e.g. viruses, fungi and bacteria, both natural and human caused. These could represent a significant confounding variable in air quality studies and we know very little about these. Another large uncertainty is the contribution of secondary organic aerosols. These seem to be increasing in several parts of the country and our understanding of the sources of these is rather limited.

Questions from Dr. Steven Packham

To the extent that I can, I have addressed some of these topics in my responses above. Since most of these questions are directly related to the epidemiology and biological mechanisms these are outside my area of expertise. Regarding the role of natural vs anthropogenic activities, I agree with the document that, on average, natural sources are relatively small fraction of the current annual standard (e.g. 12 $\mu g/m^3$). However as discussed above, these can be very important on some days and thus more relevant to the 98th percentile.