Preliminary Comments on the Policy Assessment from Dr. Michael Jerrett

Comments on Chapter 4

1. What are the Panel's views on staff's conclusions regarding support for new or updated quantitative analyses?

2. What are the Panel's views on the technical approach taken to conduct updated analyses comparing NO2 air quality to health-based benchmarks?

3. To what extent does the draft PA accurately and clearly communicate the results of these analyses? What are the Panel's views on staff's interpretation of these results for the purpose of evaluating the adequacy of the current standards?

On Chapter 4, with respect to charge question 1, the EPA staff have made appropriate conclusions with respect to the determinations related to short-term exposures. The current standards appear to be unlikely to be violated in most instances and show decreasing trends in most places. The decision not to update the short-term quantitative risk assessment appears justified.

For the long-term quantitative assessment, there may have been misinterpretations of some of the original studies, particularly the McConnell study. For example, the authors note that McConnell et al. use only the central site monitors, but they also used estimates of NOx on local streets from a dispersion model. The dispersion model had high correlations among estimates of all pollutants, but other studies have suggested that these dispersion models are strong predictors of ambient NO2 concentrations and probably weaker predictors of other confounders such as PM2.5. Thus, some large portion of the NOx prediction used in the McConnell study is likely due to NO2, and not other pollutants.

On the linearity of this effect, McConnell et al. state it is unlikely to be non-linear, so for the purpose of quantitative risk assessment, the EPA could use a linear estimate.

"There was little evidence of nonlinearity in the exposure–response relationship based on sensitivity analyses comparing the fit of a smoothed cubic spline model of asthma with a linear model (*p*-value > 0.80) for the partial likelihood ratio test for models with 3 and 5 knots compared with the linear model."

The EPA could examine Gauderman et al. 2005 paper for correlations between the NO2 measures and other predictors used in the McConnell study. Subsequent Gauderman study used data from 900 locations for NO2, NOx, NO (can get reference later). It found the biggest determinant of NO2 was traffic.

On the absence of exposure metrics, there are published land use regression models for the entire US (Marshall et al.) and for the state of California (Beckerman et al.). While these might over smooth the resulting surfaces because the models rely on government monitoring networks before the installation of the near road networks, they could potentially give a reasonable lower-end approximation of NO2 variability.

It would also be instructive to examine the correlation between PM2.5 and NO2 at the near road monitors or in high traffic areas? Ozone should not be a major issue because it is likely to be negatively correlated with NO2 and if anything this would positively confound the association. But it would be useful to examine this association as well.

On the McConnell results from the central monitors, none of the other central site exposures (PM and ozone) was significantly related to the outcome; therefore the chance that these pollutants would confound empirically would be quite low as they would not meet the definition of a confounder which must be related to both the exposure and the outcome of interest. PM2.5 was close. Much depends on how the EPA defines a likely confounder.

Thus, arguably the central site NO2 monitors could be used to assess exposures for the quantitative risk assessment.

If the EPA is concerned about lack of control for confounding co-pollutants, they could conduct sensitivity analyses that would have NO2 effects with no confounding, with 50% confounding, and 25% confounding – in calculating the likely risks to the population.

There is concern about incidence rates not being available. These rates are clearly given in the McConnell article or they can be derived from other secondary sources that give prevalence rates (which can be used to back out incidence rates).

The size of effects in the Jerrett et al. vs. McConnell et al. studies are fairly similar. Rescaling the Jerrett et al. results to the 8 ppb contrast used in the McConnell study would result in an HR of 1.39, versus the 1.51 in the McConnell study and the confidence intervals would almost certainly overlap. Thus the consistency in effects across different age groups further builds the case for causality for NO2 and suggests that the dispersion model comes close to producing the results of the individually measured home estimates.

On road estimates – more than 110,000, 000 people commute distances greater than 30 minutes per day.