DRAFT Preliminary Individual Comments on the
Health Benefits of Benzene Reductions in Houston, 1990 - 2020
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5/7/2008

Dr. Chris Walcek
State University of NY Albany

Charge Question 3: Comment on the approach of using the AERMOD plume dispersion model to estimate changes in ambient benzene concentrations.

The use of AERMOD to estimate changes in concentrations seems appropriate, and while more sophisticated models could be utilized, there are good reasons to use plume-scale models that can resolve individual plumes under conditions when small-scale local sources are important in defining areas of maximum impacts.

Some minor technical suggestions:

1) I'd like to see a map of the Benzene emissions in this study domain, and Fig. 23-24 of Appendix B are difficult to interpret since only total mass emissions are shown. Probably a fine-gridded 2-D contour plot of emissions/km^2 or something like that could be presented.

2) Meteorological data: In a coastal setting where land-sea breezes dominate local circulations, the physical location of a meteorological monitor will strongly influence the AERMOD calculations. It is noted that only two surface monitors (IAH and HOU airports) are available and the IAH monitor shows fewer missing or calm reports. The number of calm reports should not be a factor affecting the choice of data to use. Missing, yes; Calm no. I thought "Calms" were converted to 1 m/s ??? When did this ignoring of calm periods get incorporated into AERMOD? Clearly this is a biased adjustment to make.

3) Meteorological data: It is stated that a year's worth of meteorological observations are used in AERMOD, and they have 1990 and 2000 meteorology available. I would fully expect different years to yield significantly different concentrations and concentration changes. There is some discussion of apparent discrepancies in comparing 1990 with 2000 calculations that apparently arise due to using different meteorology. I would suggest doing this entire study for BOTH the 1990 AND 2000 year meteorologies. Additional meteorological information for other years should probably also be utilized somehow.

4) I'd like to see a scatter plot of predicted vs. observed benzene, and large discrepancies are expected and not undesirable. Figures in the appendix B are difficult to interpret. There is note of something like 25% of predictions being a factor of 2 low. I would expect a similar amount too high?

5) For Table 4 showing calculated benzene concentrations: Only one (maybe two) significant digits are shown. Please show more significant digits.

Additional questions that I have:

a) The costs are amortized over what time?, and why aren't sensitivity studies presented for alternate amortization times similar to the sensitivity studies using different interest rates.

b) Reductions in health impacts are termed "significant", and actual changes in mortality are presented. I think these changes should be shown in the context of the overall mortality incidence. For example, it is noted that 9 cases of leukemia are avoided over apparently a 30 year period. What is the total incidence of leukemia over this period?

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Dr. Dallas Burtraw
Resources for the Future

Charge question #2: Comment on emissions inventory

The emissions inventory is estimated in a conventional way. No original economic modeling is performed. Trends in various underlying parameters such as demographics and economic activity plus technology changes are used to forecast changes in emissions.

The narrative is detailed but it is difficult to find answers to some questions. Perhaps they are already addressed, but these issues could be brought to the fore.

1. Temperature and humidity play a role in emissions from some sources. By 2020 a "climate penalty" should apply causing greater emissions on a given projection of economic activity. Is this factored into the analysis?
2. A hierarchy of sources for emissions data for the base years 1990 and 2002 is given. In some cases information from these various sources is contrasted, but the reader cannot find a systematic evaluation of the (dis)harmony of information across these sources.
3. The personal transportation model accounts for vehicle turnover. Does this represent technological changes that would affect things like fugitive emissions?
4. It appears that a factor accounts for the entry of new nonroad vehicles into the market by accounting for sales. Does this account for changes in use and retirement of existing vehicles, and for technological differences?
5. A summary judgment is offered that missing information and general uncertainty is likely to lead to an under-representation of emissions (p. 4-2). There is some effort to discuss uncertainty, but it is not treated in an integrated fashion. Perhaps this is because there is so much uncertainty that the major effort is aimed at developing basic emission estimates. I would welcome a stronger narrative about relative sources of uncertainty and how important they are, and some discussion about how uncertainty could be better addressed in future analysis since this is a prototype.

Benzene, Charge question #5: Valuation

Questions here relate to EPA's use of a "pre-mortality morbidity" supplement to VSL for fatal cases of leukemia, EPA's development of a unit value for a non-fatal case of leukemia, and EPA's choice not to include a "cancer premium." Whether to incorporate a cancer premium may well be the most significant of the three issues raised in that the morbidity adjustments in the benzene study are comparatively small in comparison in comparison to the VSL estimate used.

Guidance provided to the EPA Administrator in 2000 by the Environmental Economics Advisory Committee indicated that estimates of the VSL derived from wage-risk tradeoff

studies should not be taken as a precise estimate of the willingness to pay for reducing the risk of fatal cancers because of differences in the nature of the risks being valued. The possibility of environmentally induced cancer, for instance, may evoke special fears, and may be seen as an involuntary hazard, whereas workers may accept workplace accident risks more or less voluntarily in exchange for higher pay. Environmentally induced cancers may also have a long latency period whereas accident risks may be seen as more immediate. Also, baseline risks of certain cancers may either be higher or lower than the baseline risks of job-related accidents (in this regard, see Chilton *et al*., *Journal of Risk and Uncertainty* 2006). Nonetheless, the committee was in general agreement that at the time, existing research did not provide a firm enough basis for selecting a quantitative "cancer premium" adjustment factor.

More recently, however, a number of studies have shed additional light on the possibility of a "cancer premium." Sunstein, in his 2005 Cambridge University Press book, *Laws of Fear: Beyond the Precautionary Principle*, argues that VSL figures should be made risk specific and suggests that the value of a life saved from cancer is at least twice the value of a life saved from a workplace accident. Hammitt and Liu (*Journal of Risk and Uncertainty* 2004) find evidence of a "cancer premium" in their contingent valuation study in Taiwan. Although the premium was only weakly significant, respondents were willing to pay about one-third more to reduce risk of an environmentally induced cancer than to reduce the risk of a chronic degenerative disease. Tsuge, Kishimoto, and Takeuchi (*Journal of Risk and Uncertainty* 2005) identify a small but significant preference for avoiding cancer risks in Japan. Van Houtven, Sullivan, and Dockins (*Journal of Risk and Uncertainty* 2008) find that willingness to pay to reduce cancer risk with a 5-year latency period is 3 times higher than willingness to pay to reduce auto-accident risks. The "cancer premium" falls when longer latency periods are considered. This study is of particular interest because the "cancer premium" estimates are computed from within subject comparisons. Finally, in a related 2007 RFF discussion paper ("Valuation of Cancer and Microbial Disease Reductions in Municpal Drinking Water"), Adamowicz *et al*. obtained a VSL for cancer of about $17 million (Canadian), which is near the high end of the range of plausible values suggested by Viscusi and Aldy (*Journal of Risk and Uncertainty* 2003).

These studies perhaps point to the eventual application of a "cancer premium", but they do not obviously provide a single scaling factor that policy makers can use today. The premium would vary with latency, baseline levels of risk under consideration and possibly other factors. Nonetheless, the possibility of a "cancer premium" in the VSL is worth additional study and the benzene report might be improved by incorporating a bit more discussion of this issue in light of recent studies. The additional discussion might be placed in p. 2-26 where the "cancer premium" issue first comes up as well on p. 4-6 when uncertainties regarding the study conclusions are discussed. For example, one discussion point might be that if a cancer premium were applied, there would be no need to add in a pre-mortality morbidity value. Presumably, this value already would be included in the "cancer premium" because it is arguably the illness length, pain, suffering, etc. that people might be willing to pay more to avoid. Another discussion

point might indicate, or at least speculate on, how large the “cancer premium” might be based on estimates provided in the literature.

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Dr. Wayne Gray
Clark University

1. General Review. I think the draft benzene case study is quite useful, although at least partly in a negative sense - a great deal of time and effort was needed to look at one air toxic in 3 counties, and even then many simplifying assumptions were needed, and the health impacts that could be quantified were small (less than one life saved per year). Given that benzene was chosen as the best-studied toxic and the study looked at an area with relatively high benzene emissions (looking under a bright but narrow lamppost), this raises questions about how well this approach could "scale-up" to measure national impacts for a wider range of chemicals. EPA's suggestion to do a simpler, back-of-the-envelope analysis to see how the results compare is a good one; it may also be worth trying a comparison of simple and detailed models in one or two other settings to see whether the quantitative results are similar - but more with a goal of improving the back-of-the-envelope approach, rather than starting on a never-ending sequence of detailed studies, one toxin at a time.

2. Emissions Estimates. Building emissions from a plant-level inventory seems reasonable, and the detailed discussion of the process in the Pechan report showed lots of effort went into preparing the inventory. The projections to 2020 based on estimates of industry growth seem less convincing. Given that a few industries are responsible for the bulk of the emissions, it might be worthwhile to pay extra attention to modeling their growth, or considering the impact of different growth assumptions. Projecting things out from 2010 to 2020 seems to have relatively little value-added: since there aren't any new regulations coming on-line, everything just carries forward (though I suppose the results demonstrate that any incremental benefits from shifts in the vehicle fleet aren't doing much).

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Dr. Morton Lippman
New York University

Charge Question #3: Air Quality & Exposure Modeling:

- Use of AERMOD for air quality modeling. This is an appropriate model.
- Use of HAPEM6 for exposure modeling. This is as appropriate a model as any.

The models were exercised appropriately, and the results appear to be sound. However, the discrepancies between the model predictions and the monitoring data suggest that the model needs more refinement.

Charge Question #4: Life Table Approach for Health Benefits:

- Selection of leukemia as primary health endpoint. For a benefits analysis, this is a logical choice.
- Use of weighted cumulative exposure measures: This was an appropriate choice.
- Interpretation of the literature on latency: The interpretation was appropriate.
- Choice of a linear dose-response function: This was the most appropriate choice.
- Sensitivity analyses: The choices made were appropriate for exposures attributable to sources amenable to EPA controls.
- No adjustment for early life exposure: This was an appropriate choice.

Charge Question #6: Analyses of Individuals in High-Exposure Environments:

- Use of a VSL approach: For a benefits analysis, this is a logical choice.
- Separate analyses for groups of individuals in high-exposure environments: It is not clear why such analyses are needed in a benefits analysis. They don't affect the bottom line.

Other Review Comments:

- The substantial underestimation of the measured concentrations by the models needs to be explicitly discussed, and not merely reported. Should an adjustment be made to the number of cases and the value of the benefits? Is the underestimation due to an inadequate emissions inventory? If not, what are the other, unaccounted for, outdoor sources of airborne benzene? If this case study model is to be applied to other HAPs and MSAs, where there will be much less data on measured concentrations, this underestimation would be a serious limitation on the credibility of the reported findings.
- The assumption of constant background concentrations over time is unwarranted, especially in view of the different background levels used for the different counties. The reductions in emissions outside the 3-county area,

which account for most of the background within them, can be expected to substantially reduce the background levels over time.

- The magnitude of the exposures to benzene from indoor sources (other than attached garages) needs to be reported and discussed in order to provide context, especially in light of the expected further reduction in outdoor sources resulting from the 2007 Mobile Source Air Toxics Rule.
- The estimated benefits, as reported for benzene in the 3-county region, are modest at best in the context of the benefits determined for ambient particulate matter (PM). In that context, does the control of benzene emissions (and those of other hydrocarbons controlled with it) cause a reduction in ambient PM that accounts for as much or more health benefits?
- If benzene is, as was assumed in calling for this benefits study, a worst case for HAPs impacts on health, is it worthwhile engaging in additional benefits studies on other HAPs? Few others will have quantitative exposure-response relationships based on epidemiological studies. Reliance cannot be based on EPA's unit-risk factors for best estimates, as aptly demonstrated in the benzene case study.

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Dr. Bart Ostro
CAL EPA

In my view, the case study used reasonable assumptions and presented a credible response to the request to provide a methodology for generating health benefits and a basis for considering the value of further HAP benefits assessment. However, I personally would have appreciated additional analysis, especially since it is quite possible that the study will be incorrectly cited as an estimate of benzene benefits. In addition some important information is used but not clearly cited in the document. Specifically, I would recommend the following:

1. Inclusion of the impact of other reductions in benzene from the MSAT Rule.

2. Citation of the unit risk factor of the Chinese worker cohort study.

3. The case study report notes several epidemiological investigations which have pointed to non-Hodgkins Lymphoma (NHL) as a cancer associated with benzene exposure. It was acknowledged that this represents an additional risk (and cost) beyond that posed by AML. However, a numerical estimate of the risks associated with NHL was not included, claiming insufficient data. There has recently been a considerable increase in the extent and quality of published data on NHL and benzene exposure (Smith *et al.*, 2007; Steinmaus *et al.*, 2008) and these should now be used to calculate these additional risks and costs for inclusion in the overall estimate.

Steinmaus C, Smith AH, Jones RM, Smith MT (2008). Meta-analysis of Benzene Exposure and Non-Hodgkin Lymphoma: Biases Could Mask an Important Association. Occup Environ Med. Apr 16, 2008 [Epub ahead of print]

Smith MT, Jones RM, Smith AH (2007). Benzene exposure and risk of non-Hodgkin lymphoma. Cancer Epidemiol Biomarkers Prev. 16(3):385-91

4. Calculation of economic benefits using current year dollars, rather than 1999 and maybe a later year for the present value.

5. Analysis of the potential co-benefits in terms of reduction in ozone and PM. Though these benefits are estimated separately, the reader has little idea as to how large the co-benefits of controlling benzene might be.

6. A discussion of relationship of current exposure to income and other measures of socioeconomic status.

7. A more detailed review, perhaps in a table, of the quantitative impact of the uncertainty factors that are cited in section 4.2, indicating the potential magnitude of the under- or over-estimation of benefits so the reader gets a feeling for their relative importance.

8. Specific recommendations for a "next step" analysis. Should additional work on benzene be undertaken or would it be more productive to consider other HAPs based on their toxicity, availability of data and amount of exposure? This would be an important input to the Council.

9. Finally, there is an issue about the Choice of study for determining the dose-response slope for AML. The use of the Pliofilm cohort is certainly "traditional" in analysis of benzene and cancer effects, but it is no longer the automatic first choice as a basis for estimating the dose response for acute myeloid leukemia (the most widely recognized and substantial cancer endpoint following benzene exposure). The Chinese study referenced in the sensitivity analysis is potentially more influential, especially given its large study population. The report of this study primarily addressed in the case study document is Hayes et *al*. (1997), which was the basis of the dose-response analysis performed by Cal/EPA in 2001. There have been a number of subsequent reports extending and commenting on the results of this study. (see *inter alia* Hayes et al., 2001; Li and Yin (2006). Some commentators have criticized the exposure measures in the earlier reports of the studies in China, but investigators associated with that study addressed these criticisms both specifically (Dosemeci et al., 1997) and by extensive additional studies of biomarkers, other hematological endpoints, chromosome aberrations etc. It is now reasonable to conclude that the most recently reported results from these studies provide the most extensive dataset on which to base benzene dose-response evaluation. In particular, they include effects following exposures to relatively low benzene concentrations, extending the validity of the assumption of linearity of response at low doses, which was a matter of uncertainty in some analyses of the Piofilm cohort.

Li G, Yin S (2006). Progress of epidemiological and molecular epidemiological studies on benzene in China. Ann N Y Acad Sci. 1076:800-9.

Hayes RB, Songnian Y, Dosemeci M, Linet M. (2001). Benzene and lymphohematopoietic malignancies in humans. Am J Ind Med. Aug;40(2):117-26. Related Articles, Links

Dosemeci M, Rothman N, Yin SN, Li GL, Linet M, Wacholder S, Chow WH, Hayes RB (1997). Validation of benzene exposure assessment. Ann N Y Acad Sci. 837:114-21.

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Dr. David Popp
Syracuse University

General

- Regarding the overall utility of this case study, it seems like a lot of work to get a small number of lives saved. Moreover, I wonder about the policy value of such a number. My understanding is that most cost-benefit analyses of the Clean Air Act find significant net benefits even without including values from HAPs. While it may be the case that such values would be useful for analysis of future regulations, the methods here apply mainly to a retrospective analysis. Given that, would a more qualitative approach be more cost-effective than such a detailed study?
- Given the small number, my intuition is that the additional health effects left out would be important (e.g. p. ES-8).
- Given the small number of leukemia found, some confidence intervals would be of value. Are these cases even statistically significant?
- On page 2-19, the study notes that death rates for leukemia have fallen since the time of the Pliofilm cohort. One would expect death rates to continue to decline. Has this possibility been considered in calculating the effect of benzene emissions, particularly in the end of the study period?

Comments on charge question #2: Emissions Estimations

I found the emissions estimations difficult to evaluate, as there is little background given about *how* benzene emissions are reduced. Is it a technological fix? Does it involve a change in fuel inputs? To what extent do benzene reductions result from changes in response to regulations on other CAA pollutants? If so, are there any anticipated technological changes that may lead to emissions reductions?

Comments on appendix A:

- p. 9: If the reduction in wood consumption from 1990-2002 results from lower natural gas prices, shouldn't future projections account for rising natural gas prices since 2002? Figure II-1 suggests this is not the case.
- Moreover, the focus on wood as a fuel source seems misplaced here? Is this much of an issue in Houston? I would expect that it isn't. Finally, in what ways do wood-burning stoves contribute to benzene emissions? That is, is this even necessary here?
- It is unclear how the growth factors between 1990 and 2000 for the no-CAA case are calculated (e.g. page 14). Since actual 2000 data include CAA regulations, what is the counterfactual used to calculate no-CAA growth factors?

- p. 41: "The differences n the distribution among the top categories could be the result of the (sic) including facilities as point sources in the 1996 NTI inventory that were aggregated and reported as nonpoint sources in the 2002 NEI." This is easily verifiable. Does the sum of point and non-point emissions from each data source agree?
- p. 51: Is the shutdown of the Champion Paper and Fibre Mill a result of CAA regulations?
- p. 66-68: Are the VMT figures for 2002 and 2009 linked to projections of energy prices in the DOE study used earlier in the appendix? That is, are the assumptions internally consistent? If energy prices go up, VMT should change. Is the study consistent about this? Moreover, doesn't the CAA affect gasoline prices (e.g. by requiring reformulated gasoline)? Do these differences in gasoline prices lead to higher VMT in the no-CAA scenarios?

Comments on charge question #6: Individuals in High-Exposure Environments

- To choose census tracts to evaluate, the study uses relative measures – the two tracts in each county with the highest level of exposure. This seems odd, as it is actual levels, rather than relative levels, that matter. Suppose, for example, that the top six counties in terms of actual exposure were all in Harris County. Shouldn't these be considered the areas of highest exposure?
- The assumption that each vehicle idles for five minutes (p. E-4) at home seems unrealistic, and presumably is important to generate the results of this analysis. Is there any external validation for this figure? If not, a lower value should be used.

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Dr. Shelby Gerking
University of Central Florida

Benzene, Charge question #5: Valuation

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