Emission Reduction Plan for Ports and Goods Movement in California (Approved April 20, 2006)

TECHNICAL SUPPLEMENT ON QUANTIFICATION OF THE HEALTH IMPACTS AND ECONOMIC VALUATION OF AIR POLLUTION FROM PORTS AND GOODS MOVEMENT IN CALIFORNIA

California Air Resources Board

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SUMMARY

This technical supplement to ARB's Proposed Emission Reduction Plan for Ports and Goods Movement provides additional information on the methods used to calculate the health impacts and economic valuation due to goods movement emissions. It includes information on the development of exposure estimates and the details on how the methodology was revised to reflect the comments received from the peer review and public comment process. Details on the development of the emission inventories are provided separately. This technical supplement is organized into the following sections:

- Exposure Estimates for Secondary Particles

This section contains exposure estimation methods for particulate matter formed from nitrate, sulfate and organic aerosols. Also, maps of monitoring data are presented.

- Calculation Protocol

The SAS program used to calculate diesel PM impacts is in this section. Also, contact information for similar codes for other pollutants is given. The factors (tons of emissions per case of health) used in calculating the health impacts due to goods movement are listed, as well.

- Peer Review Comments After 12/1/2005 and CARB Staff Responses

When the draft plan was released in December 2005, the plan was submitted for peer review to ten nationally known experts in emissions inventory development, air quality and exposure, health impacts quantification, and economic valuation. Comments from the peer reviewers and CARB's responses to those comments are provided in this section. In many cases, our approach was revised in response to their suggestions.

- Public Review Comments After 12/1/2005 and CARB Staff Responses

This section lists general public comments received on the health impacts estimation and CARB staff responses to them.

- Peer Review Comments Prior 12/1/2005 and CARB Staff Responses

This part of the technical supplement lists the scientific peer review comments on the draft methodology proposed in November 2005 and CARB staff responses.

A. Nitrates and Sulfates Aerosols

1. Nitrate, Sulfate, and Organic Aerosols Monitoring Data

The PM nitrate and sulfate data used for the exposure calculation were gathered from a variety of routine and special monitoring program databases. Ambient data from 1998 were used because that year provided maximum spatial resolution for combined routine monitoring network and special study PM data. 1998 is considered representative of present air quality because major SO_X and NO_X source emissions have not changed significantly in recent years.

The PM data that were used in this study generally met EPA's minimum data completeness criterion (11 of 15 samples per calendar quarter or no more than 25% missing data). Three different data sets for 1998 were used to provide the ambient nitrate and sulfate concentrations.

- Size Selective Inlet (SSI) high volume sampler PM10 data. In 1998 the SSI sampling network consisted of 91 sites collecting PM10 and operating on a one-in-six day sampling schedule. Data completeness screening reduced the number of sites used in this study to 60. Compositional analysis of SSI filters provides the mass of nitrate and sulfate ions.
- Children's Health Study Two Week Sampler (TWS) PM2.5 data. The TWS network was deployed to provide information for an on-going study of the chronic respiratory effects in children from long-term exposure to air pollution in southern California. Because the study required robust but not highly time-resolved data, the TWS provides continuous sample collection reported as two-week average fine particle concentrations. The two-week sampling frequency provides 26 samples per site per year and is sufficient to determine seasonal as well as annual mean concentrations. Because the TWS provides an integrated two-week measurement, and thus lacks the spikes that characterize short-term PM data, reported annual arithmetic means for TWS data were used without recalculation.
- Interagency Monitoring of Protected Visual Environments (IMPROVE) program data. The Federal IMPROVE program monitoring sites are located in federally protected Class 1 areas and are outside of urban areas. Data from 11 California sites operating in 1998 were used in this study.
- The California Dichotomous Sampler ("dichot") network data. The dichot sampler uses a low-volume PM10 inlet followed by a virtual impactor which separates the particles into two airstreams, one containing the PM2.5 (fine) fraction and the other the PM10-2.5 (coarse) fraction, with each collected on its own filter. The sum of PM2.5 and PM10-2.5 provides a measure of PM10. Samples were usually collected from midnight to midnight every sixth day.
- South Coast Air Quality Management District's (SCAQMD) Particulate Technical Enhancement Program (PTEP) data. The PTEP program operated at six sites (downtown Los Angeles, Anaheim, Diamond Bar, Rubidoux, Fontana, and San Nicolas Island) in southern California in 1995, collecting separate PM10 and PM2.5 samples.

These data were used to fill gaps in the 1998 record and to assess PM2.5 / PM10 relationships.

- California Regional PM10/PM2.5 Air Quality Study (CRPAQS) data. The purpose of the CRPAQS monitoring program was to improve current scientific understanding of excessive PM levels in Central California (Watson et al., 1998). CRPAQS is an integrated effort that includes air quality and meteorological field measurements, emissions characterization, data analysis and air quality modeling. The field program phase of CRPAQS consisted of 14 months of monitoring throughout the San Joaquin Valley (SJV) and surrounding regions, as well as intensive monitoring during fall and winter-like conditions when PM10 and PM2.5 concentrations are highest, and special summer organic measurements in Fresno. These field studies took place during late 1999 through early 2001. Air quality sampling locations in the annual network (December 1, 1999 through February, 2001) consisted of a combination of full scale "anchor" monitoring sites measuring both gaseous and aerosol species, plus supplemental monitoring sites measuring aerosol species using portable monitors at "satellite" sites, and monitors in a "backbone" network of ARB and air pollution control district sites. The annual program overlapped the episodic field programs. The winter episodic field study took place over a period of eight weeks on a forecast basis from mid-November 2000 through February of 2001.

Combining PM10 and PM2.5 Nitrate and Sulfate Data

The concentrations used in this study are a mixture of both PM10 and PM2.5 data. For annual averages, we believe that mixing PM2.5 and PM10 sulfate and nitrate data is reasonable because most sulfate and nitrate occur in the PM2.5 fraction. To confirm this, ratios of annual PM10 to PM2.5 sulfate were computed from data from the PTEP data. Ratios of annual geometric mean PM2.5 sulfate to PM10 sulfate at these sites were in the range of 0.8 to 0.9. A similar relationship between PM10 nitrate and PM2.5 nitrate has also been observed at urban locations elsewhere in California. In order to maximize spatial coverage, because the probable error is small, and because site-specific correction factors were not available for most sites, PM10 and PM2.5 sulfate data were used in this study without adjusting for which size cut was reported at each monitoring site.

Computing Sulfate and Nitrate PM Mass

Since nitrate and sulfate measurements represent only the mass of the anion, the concentration data need to be adjusted to represent the total mass of the collected particulate molecules (*i.e*. anion, cation, and associated tightly bound water). The ammonium cation (NH_4^+) is the major cation for nitrate and sulfate ions in California, so mass was calculated assuming only ammonium nitrate and sulfate were present in the samples.

There is considerable uncertainty regarding the amount of water associated with ammonium nitrate and ammonium sulfate, but, since these compounds are fully saturated when inhaled into the moist conditions within the lung, no water correction was applied. For this study, the mass associated with only the ammonium, nitrate, and

sulfate ions was computed by multiplying the nitrate values by the ratio of the molecular weight of the ammonium salt to the molecular weight of nitrate (1.29) or sulfate (1.38).

2. Calculation of Nitrates Population-weighted Exposures

In this report, staff modified the methodology used in year 2000 to address PM nitrates exposures in California. Staff updated the data base by adding monitoring data and improved the calculations to make the methodology more robust and replicable. In addition to the Statewide Routine Monitoring Network used in the previous work, staff included data from the special monitoring networks, IMPROVE and Children's Health Study (CHS), which were not available in 2000. The IMPROVE network provided additional information in the rural areas, while the CHS added more data to Southern California. Both the previous and the current methodologies were based on the Inverse Distance Weighting method. Figures 1a-c show annual geometric mean nitrate concentrations at PM monitoring sites in California. Both methods assigned weight to each monitor's annual geometric mean as a function of its distance from the point in space (for example, the centroid within each census tract) within the state, using an inverse distance weighting function (1/distance to a power). However, the power assigned to the distance was different in each method. The current methodology used a power of 2.5 in order to optimize the interpolations, whereas the previous methodology used a power of 2.0 (for distance squared). Further, the current methodology uses a minimum of 10 monitoring stations and up to a total of 15 in weighting the results to estimate the concentration at each census tract. In comparison, the previous methodology only used sites within a 50-kilometer radius, regardless of how many may fall within the fixed radius. After the interpolations were completed, the values were assigned to the affected populations within each census tract and averaged to obtain the population-weighted exposures. The previous methodology associated the interpolated concentrations to 1990 census populations while the current method uses year 2000 census. These differences account for a change in the statewide population-weighted exposures of approximately 0.45 $\mu g/m^3$ (2.25 $\mu g/m^3$ compared to previously derived value of 1.8 $\mu g/m^3$).

Figure 1a: PM Nitrates in California

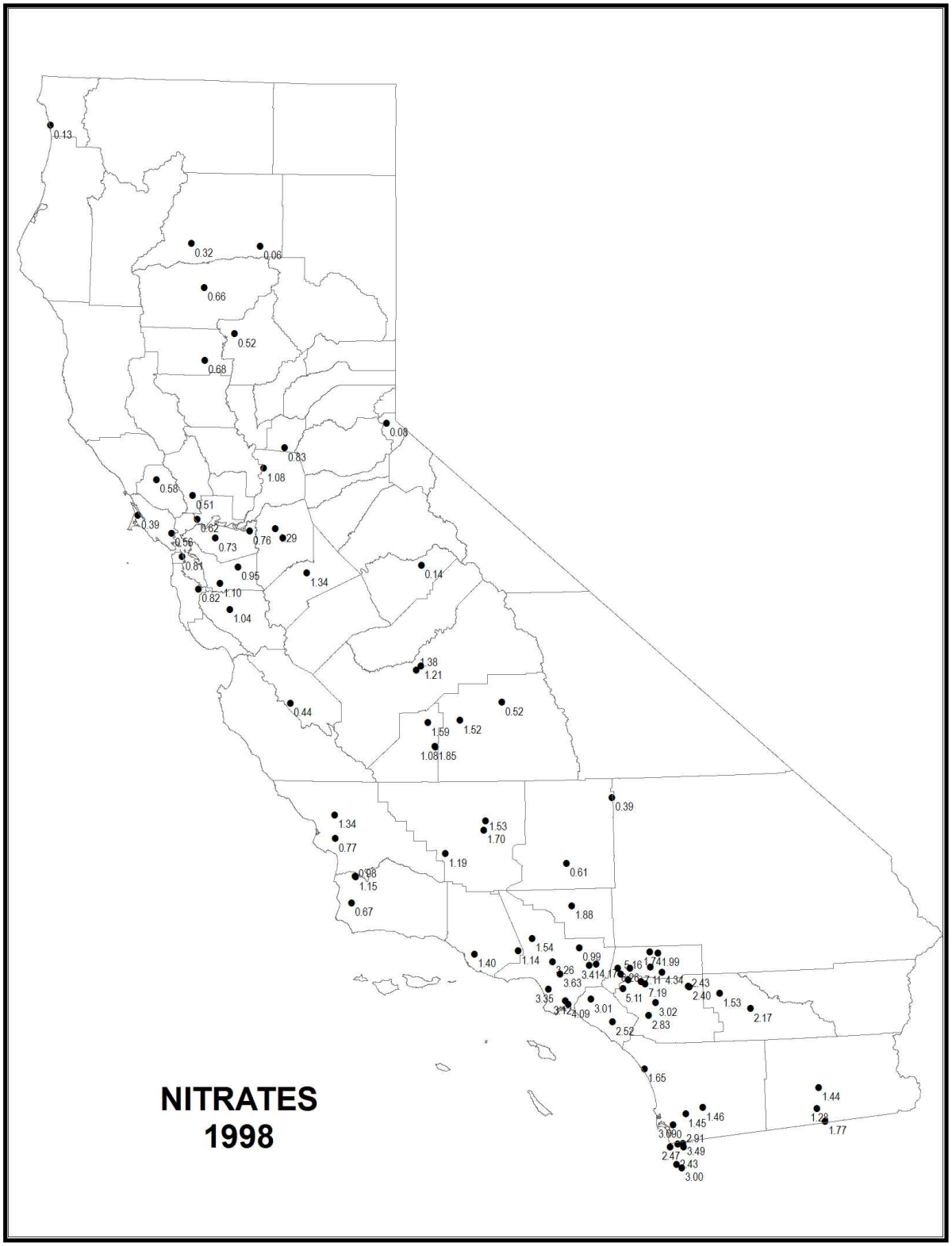


Figure 1b: PM Nitrates in Central CA

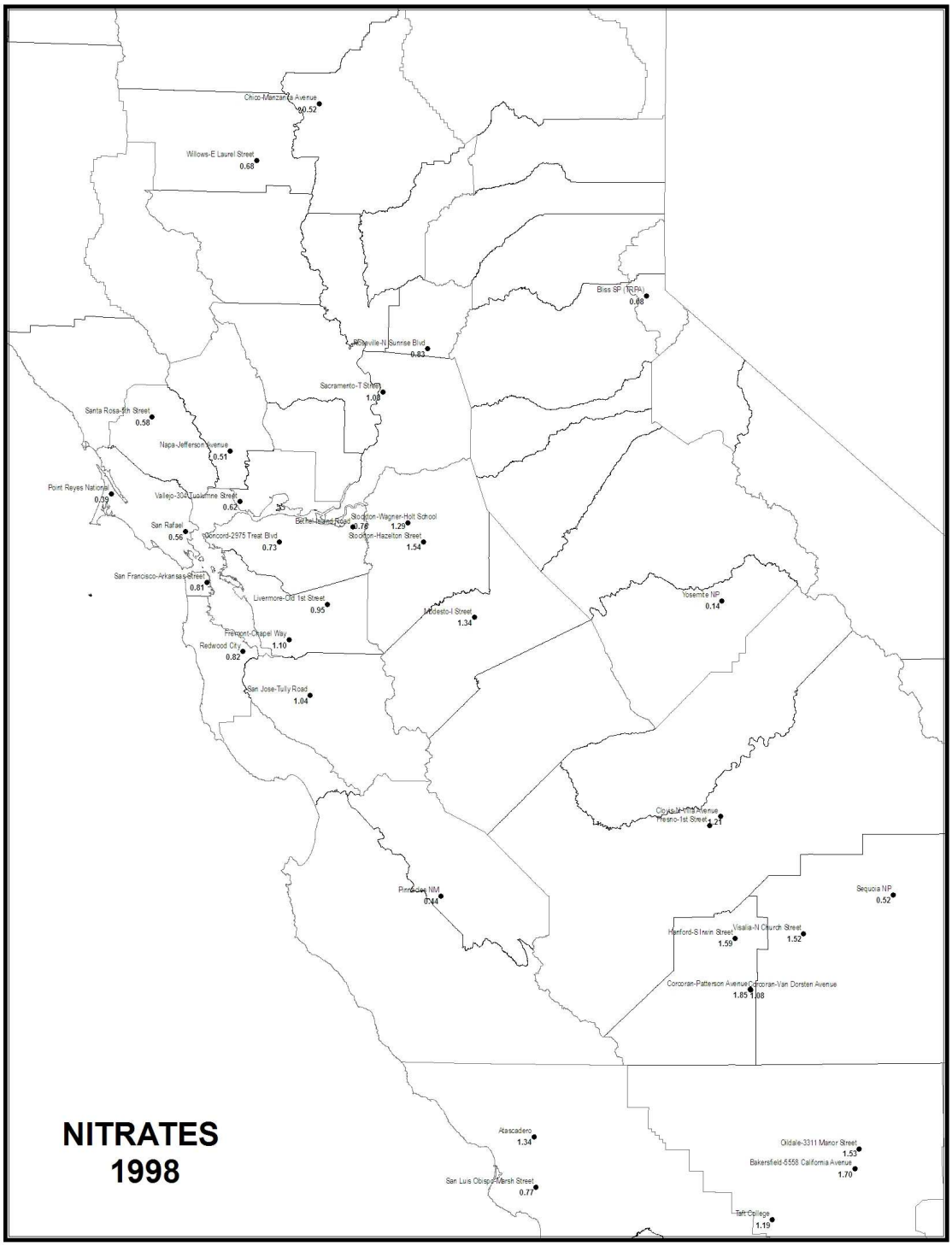


Figure 1c. PM Nitrates in Southern CA

3. Methodology of Analyses of Nitrate and Sulfate Population-weighted Exposure

a) Introduction

Population-weighted exposure is the link between ambient pollutant concentrations and pollutant concentration-response functions that permits computation of public health impacts. Population-weighted exposure is the sum of potential individual exposures computed as the product of community population and community pollutant concentration. Long term health effects for particles containing sulfate (SO_4=) and/or nitrate (NO_3-) were computed based on the annual geometric means of measured concentrations of these ions, adjusted to mass assuming that ammonium nitrate (NH_4NO_3) and ammonium sulfate ($(NH_4)_2$ SO_4) are the particulate chemical species.

This calculation is termed "potential" exposure because daily activity patterns influence an individual's actual exposure. For example, being inside a building will decrease a person's exposure to outdoor nitrate and sulfate concentrations, while a person who is outdoors may experience highly localized concentrations that are different from the community averages used in this study. Readers should bear in mind that the exposures presented here were computed to develop integrated regional values, and may not reflect all the local factors that would need to be considered to evaluate exposure at a particular location.

This exposure analysis is based solely on "outdoor" nitrate and sulfate data, as measured by the CARB and local Districts in the Statewide Routine Monitoring Network, supplemented by data from special monitoring networks such as the Federal Interagency Monitoring for Protected Visual Environments (IMPROVE) network and the Children's Health Study (CHS) monitoring program**.**

b) PM in California

Airborne particulate matter (PM) is not a single pollutant, but rather a mixture of primary and secondary particles. A large variety of emission source types, both natural and man-made, contribute to atmospheric levels of PM. Particles vary widely in size, shape, and chemical composition, and may contain inorganic ions, metallic compounds, elemental carbon (EC), organic carbon (OC), and mineral compounds from the earth's crust. PM changes as it ages in the atmosphere as directly emit PM ("primary" particles), becomes coated with the low-vapor-pressure products of atmospheric chemical reactions ("secondary" PM). Secondary PM typically contains compounds of ammonia (NH_3), oxides of sulfur (SO_X) and nitrogen (NO_X), and partially oxidized organic compounds (OC).

Generally, atmospheric PM can be divided into two distinct size classes - fine (<2.5 µm) and coarse (>2.5 µm). Fine and coarse particles differ in formation mechanisms, chemical composition, sources, and exposure relationships.

Fine PM is derived from combustion residue that has volatilized and then condensed to form primary PM, or from precursor gases reacting in the atmosphere to form secondary PM. Fine particles typically are comprised of sulfate, nitrate, ammonium, elemental

carbon, organic compounds, and a variety of trace materials usually generated as combustion “fly ash.”

Coarse particles, in contrast, are formed by crushing, grinding, and abrasion of surfaces, which breaks large pieces of material into smaller pieces. These particles are then suspended by wind or by activities such as construction, mining, vehicle traffic, and agriculture.

The spatial distribution of various PM sources, combined with diurnal and seasonal variations in meteorological conditions, cause the size, composition, and concentration of particulate matter to vary in space and time.

Sulfate

Sulfur dioxide (SO_2) emissions result almost exclusively from the combustion of sulfur-containing fuels. Other sulfur compounds, such as sulfur trioxide (SO_3), sulfuric acid (H_2SO_4) and sulfates are also directly emitted from combustion or from industrial processes, but usually in small amounts. In the atmosphere, sulfur dioxide is chemically transformed to sulfuric acid, which can be partially or completely neutralized by ammonia and other alkaline substances in the air. The dominant form of sulfate in PM in California is ammonium sulfate ($(NH_4)_2\ SO_4$). Sulfate concentrations in the SoCAB are much greater than other areas of California, and sulfate tends to be greatest during summer months due to the presence of hydroxyl radicals and other oxidants during ozone episodes.

Stringent regulations on the sulfur content of fuels have minimized sulfur emissions from most California sources, but despite low sulfur content, the large volume of motor fuel used in California still results in significant statewide SO_X emissions, of which goods movement sources such as locomotives, trucks, etc. are a significant fraction. The largest uncontrolled fossil fuel sulfur source in California is the burning of residual oil as fuel in ocean-going vessels.

Sulfate analysis is complicated by the fact that, in addition to sulfate formed from fossil fuel use in California, there are three other sources of atmospheric sulfate in California. Natural (non-anthropogenic) “background” sulfates are formed over the ocean. Secondly, global “background” sulfate is distributed throughout the Northern Hemisphere by the upper air westerly winds (potentially from sources as far as Asia and Africa and other sources). And sulfate is also blown into Southern California from combustion in Mexico. The adjustment estimate presented in this supplement is a reasonable “first approximation”, but it does not eliminate the need for more detailed study of SO_X transport across the border in southern California. Also, new analyses of air quality and emissions data conducted since December 2005 indicate that uncontrolled SO_X emissions from ships increase the estimates of total goods movement-related health effects by about one quarter. However, this preliminary estimate contains several uncertainties (discussed in more detail in section A.5) .

For these reasons, we did not quantify the health impacts from exposures to sulfates. Additional research is underway to reduce the uncertainties associated with the current analysis. The research includes a refined inventory of ship activity and ship emissions, analysis of historical PM data from sites along the West Coast of northern America to

look for evidence of ship emissions, and development of new monitoring methods that can distinguish fossil fuel sulfate from that due to biologic activity in the ocean. Further, a model is also being developed to allow simulation of sulfate formation and transport over the ocean and land areas of coastal California.

Nitrate

In urban areas of California, nitrate represents a larger fraction of PM mass compared to the rest of the nation due to the State's widespread use of low-sulfur fuels for both mobile and stationary sources. The formation of secondary ammonium nitrate (NH_4NO_3) begins with the oxidation of oxides of nitrogen (NO_X) into nitric acid (HNO_3). The nitric acid then reacts with gaseous ammonia to form ammonium nitrate (NH_4NO_3).

In coastal areas, gas phase acids can react with sea salt by reaction of nitric acid (HNO_3) with sea salt particles (NaCl), producing stable particulate sodium nitrate ($NaNO_3$) accompanied by liberation of gaseous hydrochloric acid (HCl). This reaction is a principal source of coarse nitrate, and plays an important role in atmospheric chemistry because it is a permanent sink for gas-phase nitrogen oxide species.

Geometric Mean Mass

Particle concentration data commonly exhibit a skewed frequency distribution, with many low values and a few very high ones. For this reason it is standard practice to treat these data as log-normally distributed, and thus annual concentration statistics are reported as a geometric mean, which provides a better representation of "typical" concentrations than would an arithmetic mean.

4. Calculation of Nitrate and Sulfate Population-weighted Exposures

Concentrations of many air pollutants, including nitrate and sulfate, change substantially from place to place. Accordingly, population exposure estimates tend to be more accurate when the population data and air quality data on which they are based are highly geographically resolved. Population counts by census tract group block (typically a few thousand people) provide a convenient source of highly resolved population data. Densely populated areas have many census tract group blocks, while sparsely populated areas have very few.

In order to compute a population-weighted exposure, the scattered measurements of PM must be converted to a form that allows assigning annual PM concentrations to all populated areas of the State. This was done using the Inverse Distance Weighting method implemented in the *Geostatistical Analyst 9.0* software package to interpolate PM concentrations down to the census block level. The nitrate and sulfate annual geometric mean values and population counts were associated by census tract group block and merged to assemble a spatially resolved population-weighted exposure estimate.

The interpolation procedure for assigning nitrate and sulfate concentrations to a census tract group block computed a weighted-average of the concentrations measured at 10 or more neighboring monitors. The weight assigned to each monitor was a function of its distance from the point being estimated, using an inverse distance weighting function of $1/d^2$. Using a weighting exponent of 2 forced the estimates to be strongly weighted to the closest monitors. For most points a minimum of 10 monitoring stations were used,

with up to 15 used for some locations. Geographical barriers such as mountain ranges that may impede the movement of emissions and pollutants were not considered in the exposure calculations. While this may cause some rural estimates to be less accurate, this omission had little impact on the overall results since strongly weighted local monitors were available to drive the estimation for most of the State's population.

5. Background Estimation for PM Nitrate and Sulfate

Background Estimation for PM-Nitrate

PM nitrate is generated from local emissions by a reversible chemical reaction that is dependent on temperature, relative humidity, and the concentrations of the precursor gases (ammonia and nitric acid). Long range transport of nitrate is generally weak because dispersion, heating, or drying of the air mass will cause ammonium nitrate to break down and return to its gas phase components. Small amounts of non-volatile nitrate can form by reaction of nitric acid with soil or sea salt, but limited measurements suggest that "background" concentrations are very low (generally less than 0.1 $\mu g/m^3$). For this reason, no effort was made to adjust measured nitrate values for a background contribution.

In general, the volatile nature of nitrate makes it generally short lived, and thus there is little "background" nitrate in the free troposphere. Some stable nitrate is formed by reaction of nitric acid with mineral dust (Gong *et al*., 2003), and there is a small amount formed in the marine boundary layer by reaction of natural nitric acid with sea salt (in the shore zone in populated areas, most of this reaction is driven by NO_X emissions from anthropogenic sources).

There is little information on global nitrate except as generated by specialized transport models (Gong *et al*., 2003). Published PM background observations in California for global-scale transport (VanCuren, 2003) show tropospheric background nitrate to be on the order of 0.1 – 0.2 $\mu g/m^3$ PM2.5. Nitrate measurements at Trinidad Head associated with strong on-shore winds (to suppress local NO_X emission effects) are less than 0.1 $\mu g/m^3$ PM2.5. These values are comparable to those reported in transport models (Gong *et al*., 2003).

Based on these observations, nitrate values used in this study were not corrected for nitrate from global transport and oceanic processes.

6. References

Gong, S. L., X. Y. Zhang, T. L. Zhao, I. G. McKendry, D. A. Jaffe, and N. M. Lu, Characterization of soil dust aerosol in China and its transport and distribution during 2001 ACE-Asia: 2. Model simulation and validation, *J. Geophys. Res.,* 108(D9), 2003.

VanCuren, R., Asian aerosols in North America: Extracting the chemical composition and mass concentration of the Asian continental aerosol plume from long-term aerosol records in the western United States, *J. Geophys. Res.*, 108(D20), 2003.

Background Estimation for PM-Sulfate

Stringent regulations on the sulfur content of fuels have minimized sulfur emissions from most California sources, but despite low sulfur content, the large volume of motor fuel used in California still results in significant statewide SO_X emissions, of which goods movement sources such as locomotives, trucks, etc. are a significant fraction. The largest uncontrolled fossil fuel sulfur source in California is the burning of residual oil as fuel in ocean-going vessels. Sulfate analysis is complicated by the fact that, in addition to sulfate formed from fossil fuel use in California, there are three other sources of atmospheric sulfate in California – natural “background” sulfate formed over the ocean, global “background” sulfate that is distributed throughout the Northern Hemisphere by the upper air westerly winds, and sulfate blown into Southern California from combustion in Mexico.

Background concentrations are those that would be observed in the absence of anthropogenic emissions of particulate matter (PM) and its precursors. Characterizing the background is necessary to determine the exposure and risk associated with regional anthropogenic emission. As emissions continue to be reduced due to the use of cleaner fuel and control technologies, the issue of specifying the background to the exposure of airborne particles has become increasingly important in the regulation of pollutant emissions in the United States. A survey of global data indicates that the concentrations of fine particles and their chemical composition are spatially and temporally highly variable in remote areas that intrinsically are presumed to be dominated by natural particle emissions. Adding to the ambiguities in defining the background for aerosol particles is the recognition that intercontinental baseline conditions are affected by regional-scale events, including long-range air mass transport.

In California, background monitoring sites are intended to quantify regionally representative PM concentrations for sites located away from populated areas. "Background" is not a single value; local geographic conditions such as annual rainfall, exposure to the ocean, and other factors cause PM concentrations and particle components in remote locations to be regionally variable.

Sulfate is formed by atmospheric conversion of gaseous sulfur emissions to sulfuric acid and then to a stable salt (usually ammonium sulfate). Gaseous sulfur emissions from fossil fuel combustion (SO_X) are due to naturally occurring sulfur contamination in fossil fuels. Much of the airborne sulfate in California is due to anthropogenic sulfur emissions, but apportioning exposure to sulfur sources must take into account “background” sulfate from the two major exogenous sources of sulfate in California - biogenic sulfate generated over the ocean, and regional–to-global scale transport of natural and anthropogenic sulfate in elevated layers of the atmosphere.

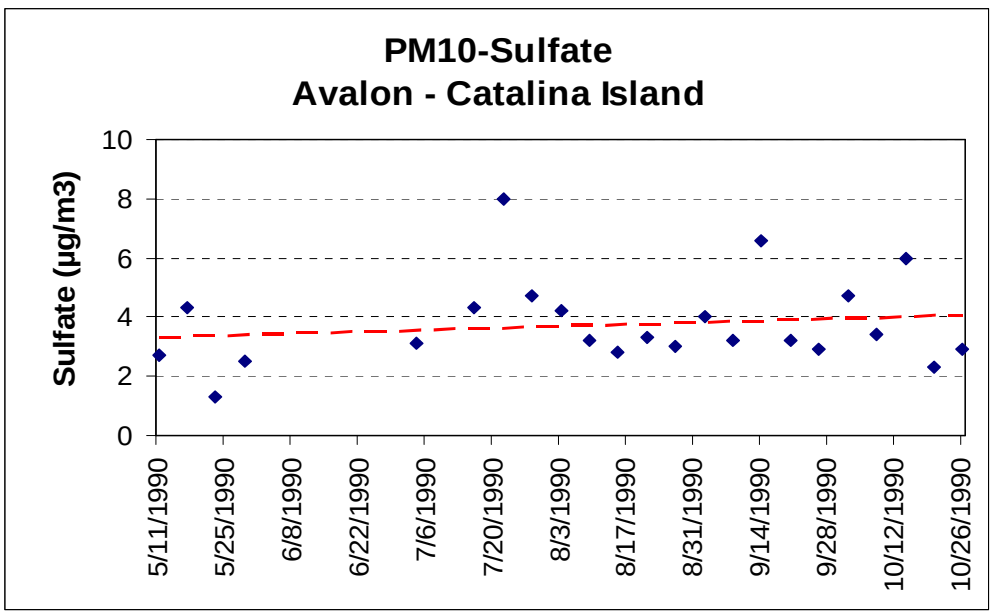
To assess a range of values representative of PM-sulfate background levels throughout the State, we used the average of the measurements obtained at several sites located in the most pristine areas in California, and performed a quantities examination of the relationship between sulfate air quality and secular oxide emissions.

To estimate background sulfate several approaches were taken:

a. Southern California

1. In Southern California, background was estimated by comparing literature values for marine biogenic sulfur and limited sulfate monitoring data from San Nicolas and Santa Catalina Islands. The 1995 annual average PM10-sulfate at San Nicolas Island was about 2 µg/m^3. The mean spring-to-fall sulfate concentration was 3.75 µg/m^3 measured at Avalon on Santa Catalina Island (Figure 2). Several sulfate events with increased sulfate concentrations were also measured at this site, suggesting that observed sulfate at these sites includes anthropogenic pollution. A tracer study by Shair et al. (1982) examined the fate of materials transported seaward by the land breeze. This study concluded that some of the high pollutants events in Santa Catalina Island could be due to circulation of Southern California emissions (both on-land and offshore). However, more field measurements and data analysis are needed to characterize the changes in the annual average concentrations, as well as the seasonal changes, and to identify sulfate sources. Based on the lower range of Catalina data and the San Nicolas annual mean, background concentration at the shoreline in Southern California is estimated to be 2 µg/m^3 annual average.

Figure 2. Temporal variation of PM10-sulfate concentrations at Avalon - Catalina Island.



2. Oceanic sulfate concentrations are expected to decrease as the air mass moves inland due to deposition and other loss processes. Inland "background" concentrations were estimated by statistical analysis. The strong spatial consistency of sulfate concentrations (Figures 11 and 12) indicates that sulfate processing in the South Coast Air Basin (SoCAB) is a highly organized, repeating process. Assuming that all SO_X emission changes are distributed uniformly (i.e., the relative spatial distribution of emissions remains fixed), there should be a simple linear relationship between areal emissions and observed ambient sulfate concentrations. One can assume scenarios (i.e., theoretical arguments) where the relationship is non-linear, but the long-term empirical record shows that

ambient sulfate concentrations respond linearly to changes in SO_X emissions. The relationship between ambient sulfate data and sulfur oxide emissions were evaluated using ordinary least squares linear regression. The procedure consists of fitting the annual mean sulfate concentrations versus year for the period of 1985 through 2000 and then regressing these smoothed annual concentrations against the regionally representative emission inventory for the base emission inventory years (1985, 1990, 1995, and 2000 – baseline emission inventory is updated every 5 years). In order to obtain an uniform spatial distribution of emission, the estimated annual average emission (tons per) was converted to micrograms, then it was divided by area of county or basin (region in question), here after it is referred to areal adjusted SO_X emissions. This presentation is a quantitative examination of the relationship between SO_X emissions and ambient sulfate concentrations, similar to the approach employed by Husar and Wilson (1993), Schichtel et al. (2001) and Malm et al., (2002). No specific attempt was made to account for meteorological forcing, i.e., space/time variation in air mass transport, pollutant transformation, and removal rates as well as the seasonal variation in emissions. However, these effects were minimized by aggregating sulfate data over long periods of time and over large regions. The intercept of this linear fit at zero emissions is interpreted as an upper bound for local sulfate background concentrations.

Annual mean sulfate data are plotted as a function of year, and against areal sulfur dioxide emission rates (Figures 3a-b and 4a-b) at Riverside and El Toro (Orange County). They show a linear relationship, with ambient sulfate decreasing as SO_X emission decreased, similar to that of other studies conducted across a large array of atmospheric conditions. Both sites show non-zero intercepts which represent the amount of sulfate that is due to sources not in the local emission inventory. These intercepts of about 1.7± 0.64 µg/m^3 at Riverside and 3.1±0.19 µg/m^3 at El Toro can be interpreted as an approximate annual mean for natural sulfate plus exogenous anthropogenic sources at those sites.

El Toro is relatively close to the coast and is expected to be impacted by emissions from the Los Angeles/Long Beach area to the North. At this site it is reasonable that the oceanic “background” will not have decreased significantly and additional sulfate from urban sources would contribute to the intercept. This is consistent with the mean “background” sulfate at Avalon. Riverside is much further inland, so the lower intercept is interpreted as dilution of “background” by a factor of 2 compared to coastal sites. At Riverside, sulfate carried by the sea breeze is reduced by deposition and diluted by dispersion as the air moves inland. The excess over the natural sulfate of 1 µg/m^3 and 0.5 µg/m^3, respectively, is consistent with transport of sulfate from upwind areas. Figures 5a shows that the estimated background sulfate is about 2 µg/m^3 at the coast, and decreasing to 0.75 µg/m^3 at inland sites such as Banning and Redlands. These estimates provide guidance for evaluating the impact of terrain, meteorology, and distance from the ocean for other sites in Southern California. Figure 5b shows sulfate background and observed ambient sulfate concentrations at monitoring sites in California.

Figure 3a. Annual trends in PM10 –Sulfate concentrations at Riverside-Rubidoux.

Figure 3b. Annual mean ambient sulfate concentrations versus SO_X emissions at Riverside –Rubidoux.

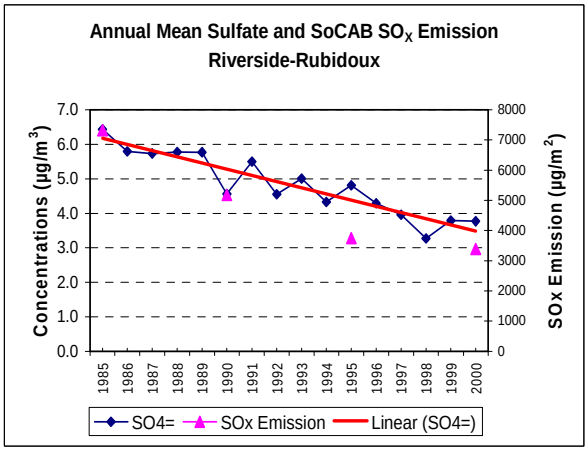


Figure 3a.

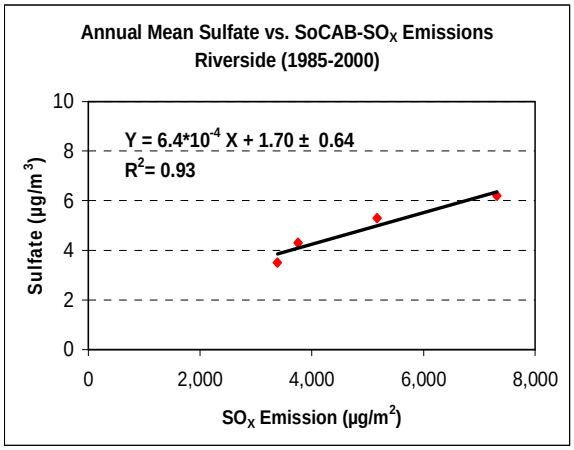


Figure 3b.

Figure 4a. Annual trends in PM10 –Sulfate concentrations at El Toro.

Figure 4b. Annual mean ambient sulfate concentrations versus SO_X emissions at El Toro.

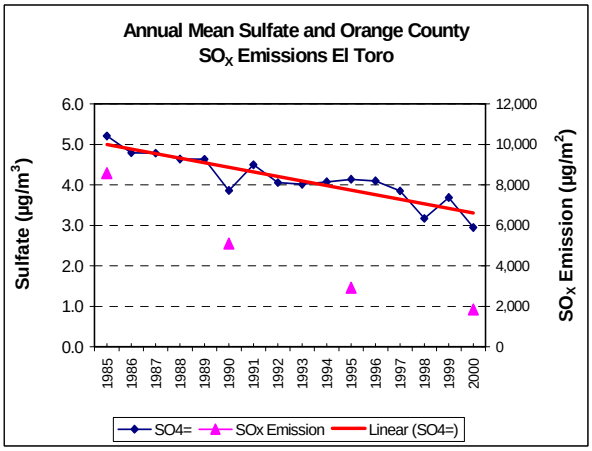


Figure 4a.

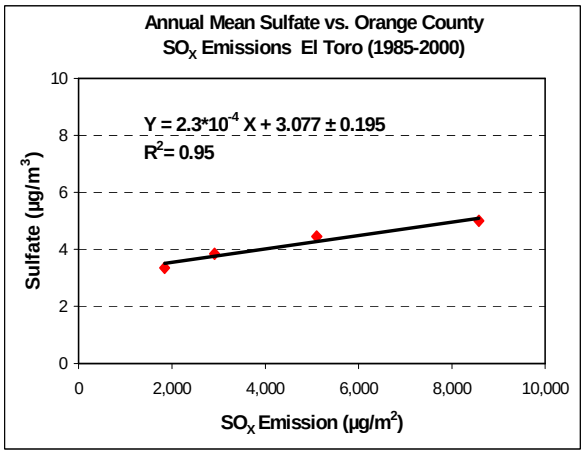


Figure 4b.

Figure 5a. Sulfate background and observed annual average ambient sulfate concentrations at monitoring sites in Southern California.

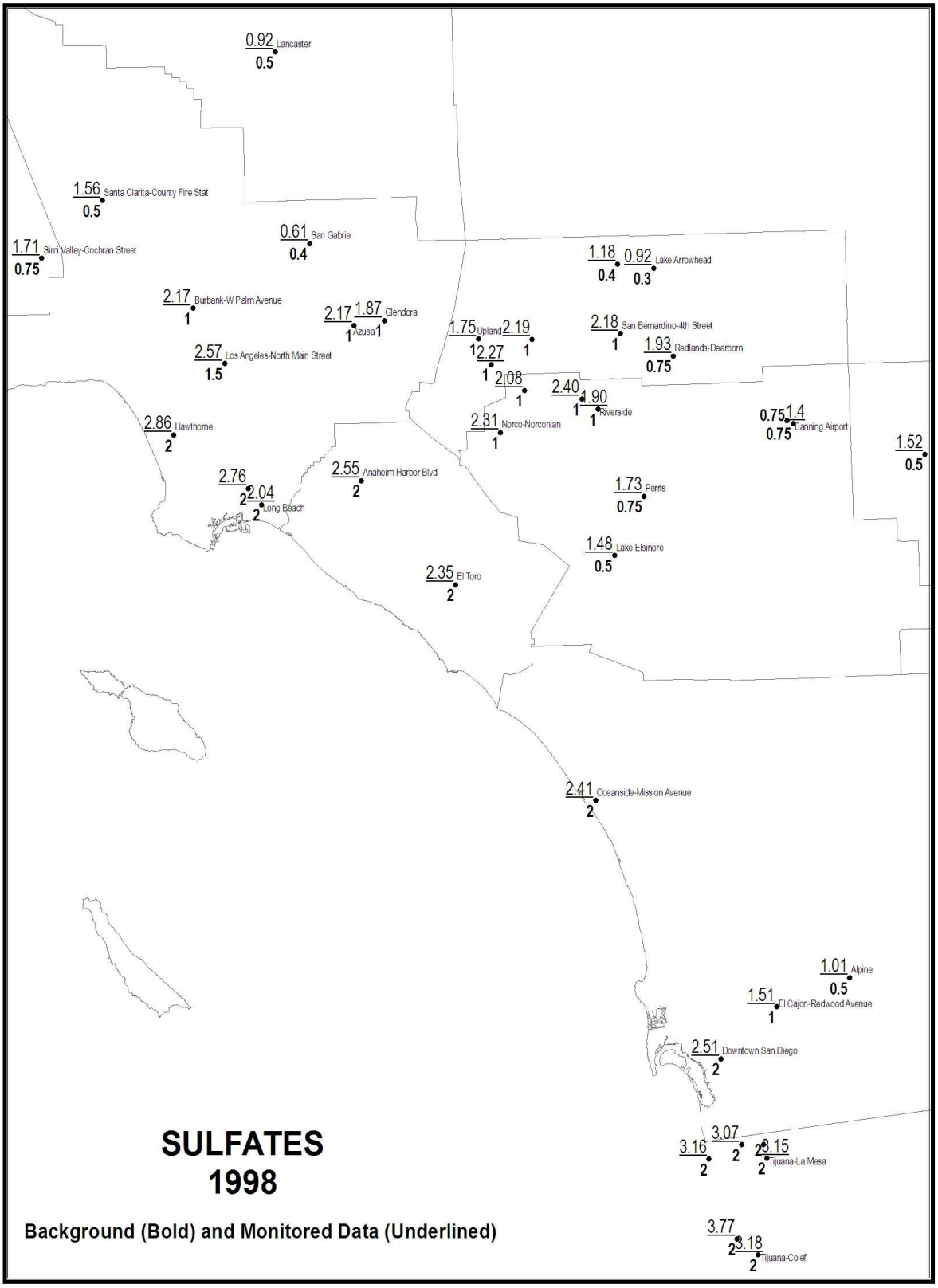
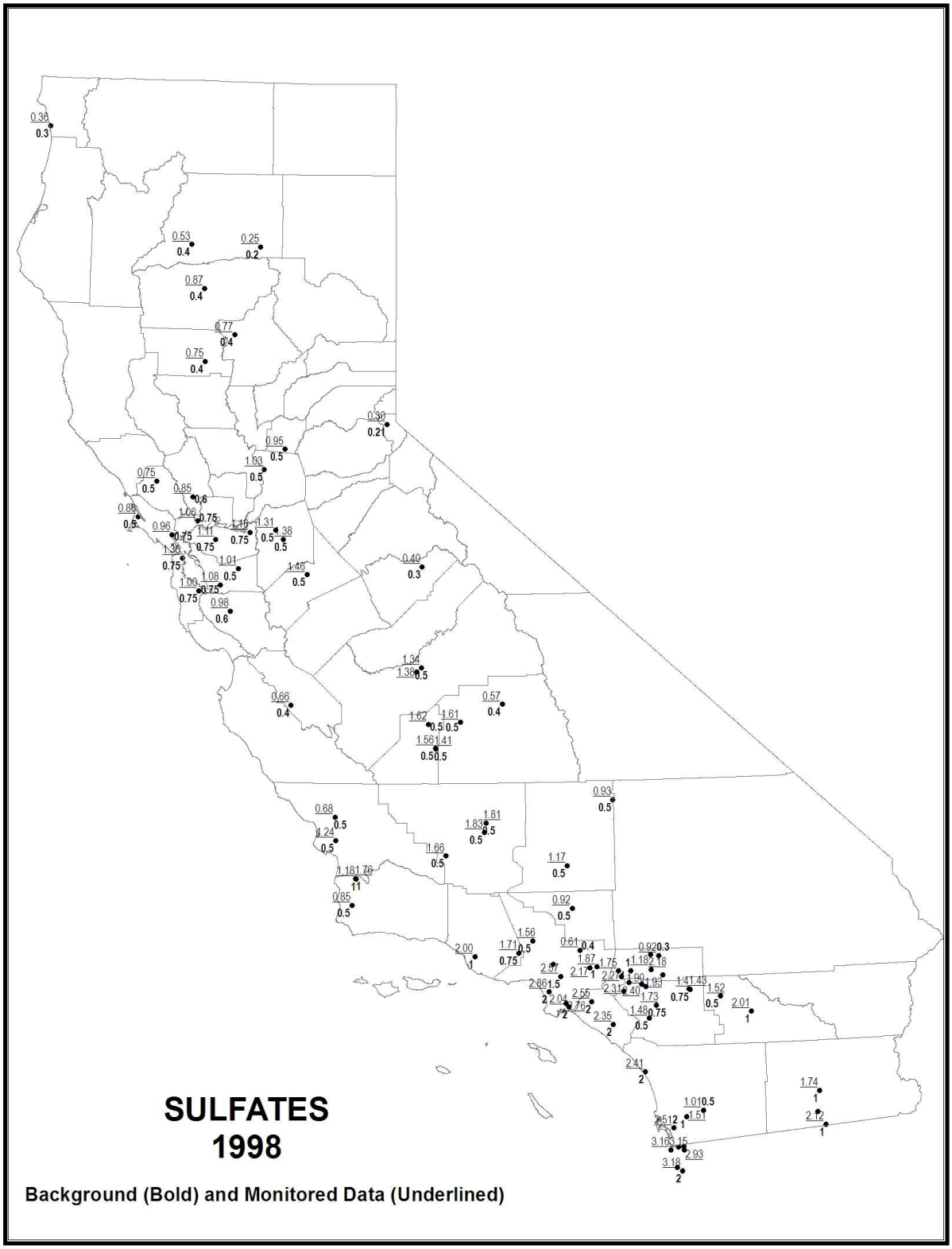


Figure 5b. Sulfate background and observed ambient sulfate concentrations at monitoring sites in California.



b. Northern California

Similar regressions for ambient sulfate and area adjusted SO_X emissions were calculated for sites in Napa County, Marin County, the Sacramento-San Joaquin Delta, and Alameda County (Figures 6a-d). Moderately good correlation was observed with an intercept of 1 µg/m^3 for the first three sites. Fremont's slightly higher intercept of 1.27±0.33 µg/m^3 may be due to additional anthropogenic sources in the East Bay. These graphs indicate that annual average sulfate levels would increase linearly above mean background concentrations when plotted against annual average sulfur dioxide emissions. A background sulfate level will exist due to natural sources as well as to man-made emissions external to the region. A lower oceanic sulfate concentration of 1.0 µg/m^3 in Northern California is comparable with the spring mean PM10 sulfate data observed at Trinidad Head of about 1.6 µg/m^3 since most coastal sites do not have continuous sea breezes. As in Southern California, background sulfate concentrations decrease with increasing distance from the coast.

Figures 6a-d. Annual comparison of ambient sulfate versus SO_X emissions at 4 sites in San Francisco Bay.

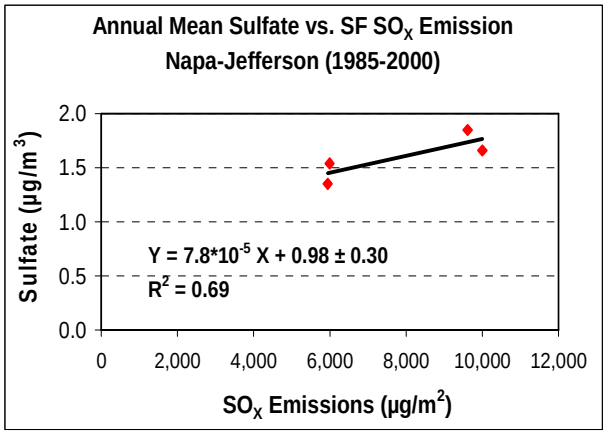


Figure 6a.

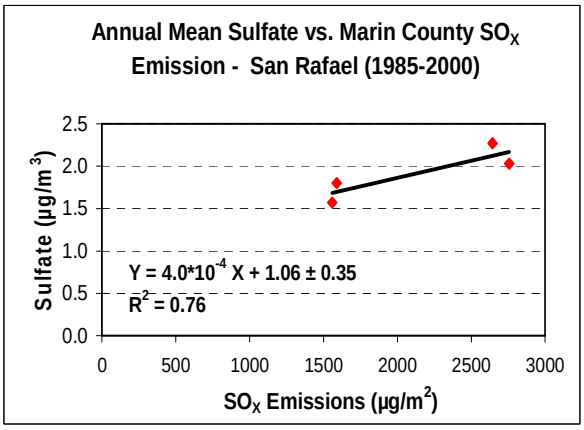


Figure 6b.

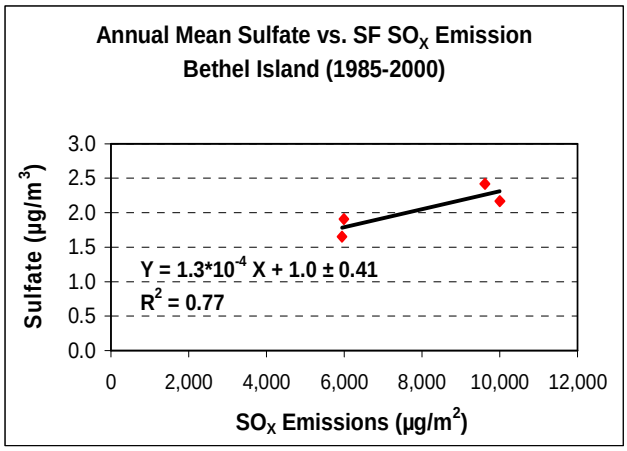


Figure 6c.

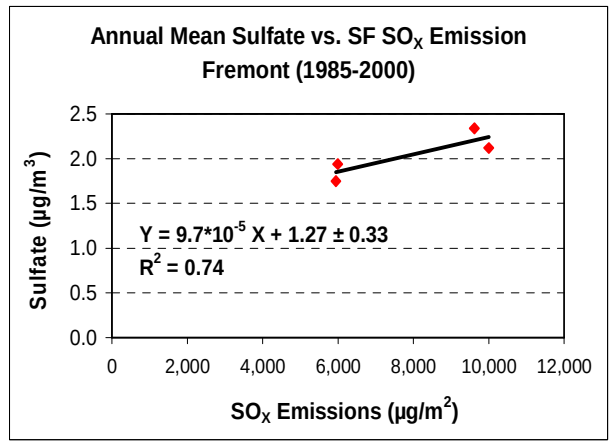


Figure 6d.

Unequivocal measurements of background sulfate are limited to a few weeks of data from three sites in northern California. Oceanic sulfate data come from Trinidad Head in Humboldt County, and global transport sulfate data come from Trinity Alps and Mount Lassen. Comparison with several years of routine monitoring data permitted estimation of the average annual concentration of background sulfate at these sites. 1.6 µg/m^3 of sulfate was observed at Trinidad Head, a site on the coast which should have small anthropogenic contribution. Further inland at high altitude sites in the Trinity Alps and Mount Lassen the background sulfate is approximately 0.25 µg/m^3. This value was used for all similar high altitude and rural areas of the state.

c. <u>Interior California</u>

Unlike coastal or high mountain areas, where background sulfate estimates are anchored to measurements, interior lowland sites have neither well-known sulfate sources nor remote, unpolluted measurement sites from which to extrapolate concentrations to populated areas. Interior background sulfate was estimated by a modified roll-back procedure applied at sites where the regional sulfate source areas could be reasonably identified. Results for a limited set of sites were used to calibrate statewide estimation of background sulfate concentrations.

Three such sites are discussed here: Redding, Oildale, and Mojave.

Redding is in a semi-enclosed basin at the northern end of the Sacramento Valley, surrounded by mountains to the west, north, and east. Transport from upwind areas to Redding, when it occurs, is predominantly from the lower Sacramento Valley. To test for sensitivity to transport, the regressions were performed for both the Shasta County (local) and Sacramento Valley Air Basin emission inventories. Based on emission data from Shasta County, the Redding site shows a non-zero intercept of 0.53±0.09 µg/m^3 which represents the amount of sulfate that is due sources not in the local emission inventory. The fitted time trend and regression are shown In Figures 7a and 7b.

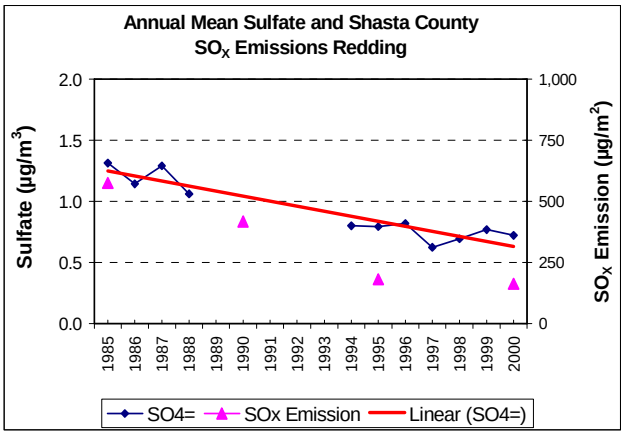


Figure 7a.

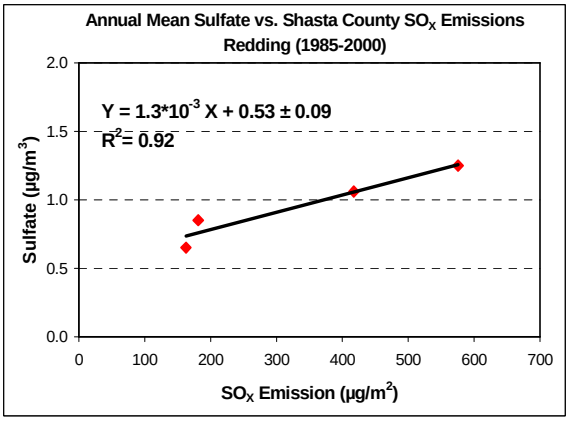


Figure 7b.

Oildale is in a semi-enclosed basin at the southern end of the San Joaquin Valley, surrounded by mountains to the west, south, and east. Transport from upwind areas to Oildale, when it occurs, is predominantly from the northern San Joaquin Valley. To test for sensitivity to transport, the regressions were performed for both the Kern County (local) and San Joaquin Valley Air Basin emission inventories. Based on emissions from the SJV portion of Kern County, the Oildale site shows a non-zero intercept of 1.0±0.4 µg/m^3 which represents the amount of sulfate that is due sources not in the local emission inventory. The fitted time trend and regression are shown in Figures 8a and 8b.

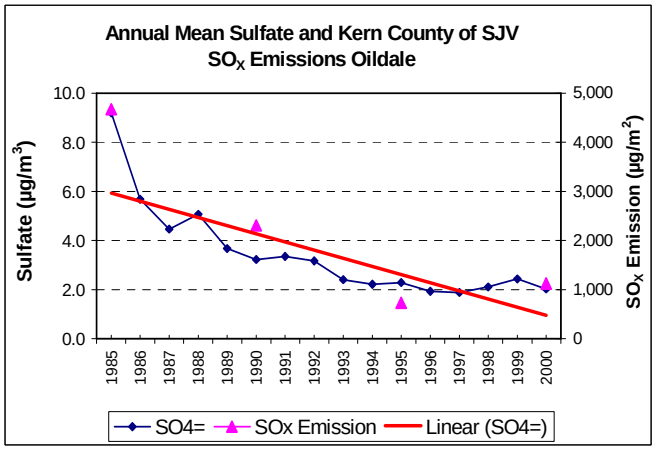


Figure 8a.

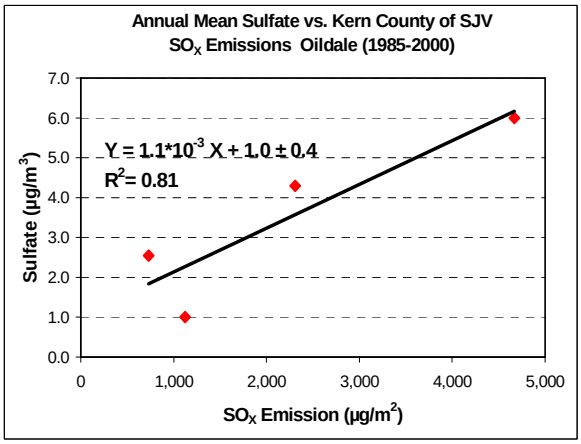


Figure 8b.

Mojave is located in the Mojave Desert Air Basin portion of Kern County, near the eastern end of Tehachapi Pass. Pollutant transport studies (White and Macias,1991; Green et al, (1992a, 1992b, 1993) have shown that Mojave air quality is dominated by transport from the San Joaquin Valley through Tehachapi Pass and from the SoCAB through Soledad Canyon and Tejon Pass. Regressions were run for Mojave for local,

Kern County (SJV and MD Air Basins), and total San Joaquin Valley influences. The regression for total San Joaquin Valley emissions indicates an intercept of about 0.86±0.14 µg/m^3 at Mojave. The fitted time trend and regression are shown in Figures 9a and 9b.

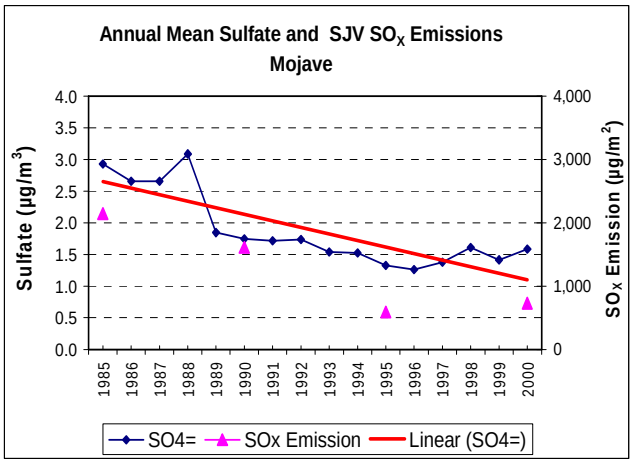


Figure 9a.

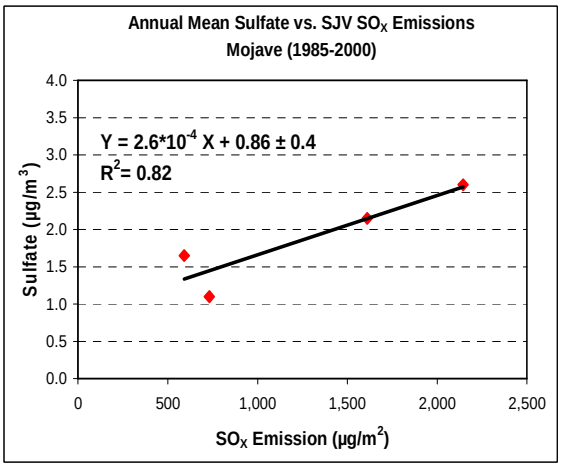


Figure 9b.

Based on these regression analyses, lowland interior background sulfate is estimated to be on the order of 0.4-0.5 µg/m^3.

Similar regressions for ambient sulfate and area adjusted SO_X emissions were calculated for four sites in Salton Sea Air Basin sites (Figures 10a-d). Strong correlation was observed with an intercept of about 2 µg/m^3 for these four monitoring sites.

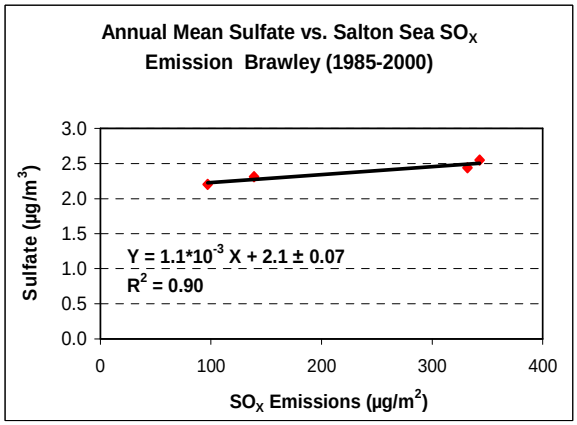


Figure 10a.

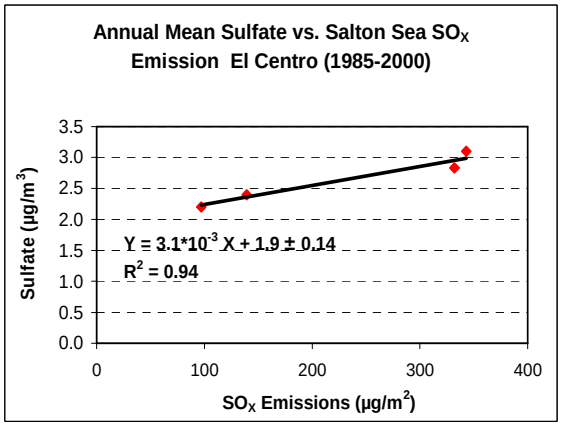


Figure 10b.

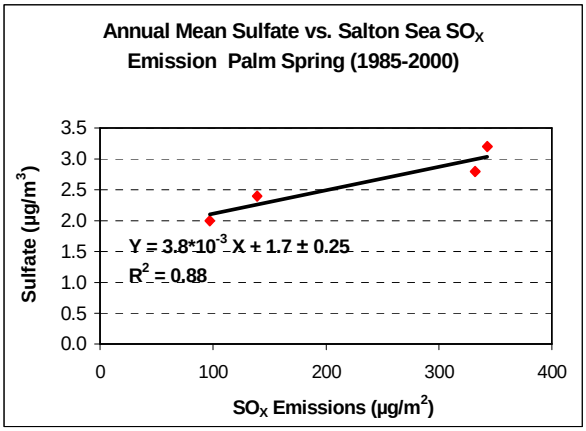


Figure 10c.

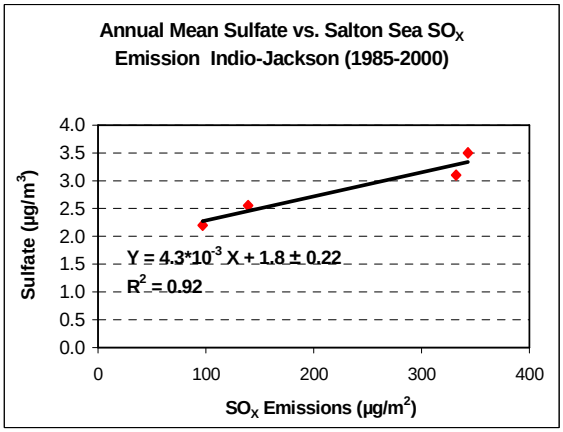


Figure 10d.

These intercepts of 1.7 to 2.0 µg/m^3,at Brawley, El Centro, Palm Spring, and Indio Jackson, are assumed to represent an approximate annual mean for natural "background" sulfate plus exogenous anthropogenic sources at those sites.

Computations of annual average background sulfate in the rest of the State were based on approximations of the effects of site-specific meteorology and terrain on inputs from the ocean (both sulfate and precursor gaseous sulfur compounds) and upper air. The statewide estimates were reviewed for consistency with reported sulfate air quality data and published global sulfate model results (Hidy and Blanchard 2005). Finally, the background estimates were subtracted from ambient data to approximate site-specific anthropogenic sulfate concentrations. Our estimate of sulfate background level is consistent with Hidy and Blanchard's estimate using data from the remote-rural IMPROVE network. Although there is considerable uncertainty in the background estimates, ambient concentrations at most urban sites in California are several times background, so that the impact of this uncertainty on statewide sulfate population exposure is believed to be small (about ½ µg/m3).

d. <u>Adjustment for Sulfate Transport from Mexico</u>

The metropolitan area of San Diego is the third largest in California with a population of more than 2,000,000. Although the climate is similar to that of the Los Angeles metropolitan area, the air quality in the San Diego area is better, primarily because of different topography and emission sources. There are no major industrial or utility sources of pollutants near the Pacific Coast in San Diego County, and the main local source is the motorized transportation. However, the San Diego harbor is a busy port serving both civilian and military vessels.

Stringent regulations on the sulfur content of motor fuels in California, widespread replacement of fuel oil with natural gas as boiler fuel, and very little coal use have combined to minimize sulfur emissions from most California sources. Despite low sulfur content, the large volume of motor fuel used in California still results in significant statewide SO_X emissions. The largest uncontrolled fossil fuel sulfur source in California is the burning of residual oil as fuel in ocean-going vessels.

Prior work suggests that shipping emissions can have significant local, regional, and global environmental impacts (Davis et al., 2001; Corbett and Koehler, 2003; Endresen et al., 2003). Moreover, ships are increasing in number and size, while the residual heavy fuel oil they use is degrading in quality (Murphy et al., 2003). Murphy et al. (2003) estimated that in 1999 marine shipping activities contributed 36 percent of the total NO_x emissions in Santa Barbara County, California, and will constitute about 60% by 2015 - five times the emissions associated with on-road motor vehicles.

Luria et al (2005) indicate that the majority of SO_2 in the region is transported from sources south-southeast of San Diego, most likely from Mexico. In this study, the ratio of SO_2/NO_Y suggests emissions are from sources with limited or no controls. Several other studies also confirmed that sulfate formed from precursor emissions from northern Mexico could contribute to elevated concentrations in near-border areas of California. Since Mexican sources are not included in the California inventory, they constitute an additional exogenous sulfate fraction that, like background, needs to be removed before computing source-category pollutant exposures based on ambient monitoring data.

To estimate the impact of local sources on sulfate concentration in San Diego, SO_X emissions and sulfate ambient concentrations in Orange and Riverside counties were examined. These counties are further away from Mexican sources and provide a model for looking at the relationship between emissions and ambient concentrations in the absence of theses sources.

The repeating nature of sulfate processing can be seen in sulfate data from the Riverside Rubidoux monitoring site plotted against matched sulfate data at Anaheim and Mission Viejo (Figures 11 and 12). The regression of 24-hour sulfate concentrations indicates a relatively strong link between sulfate formation and transport processes at these coastal sites and Riverside-Rubidoux, more than 100 km from the coast. There seems to be a relatively good mixing of sulfate throughout the basin, which might be consistent with on-shore and off-shore flows over a 24-hour period.

Figure 11. PM10 Sulfate comparison at Riverside and Anaheim.

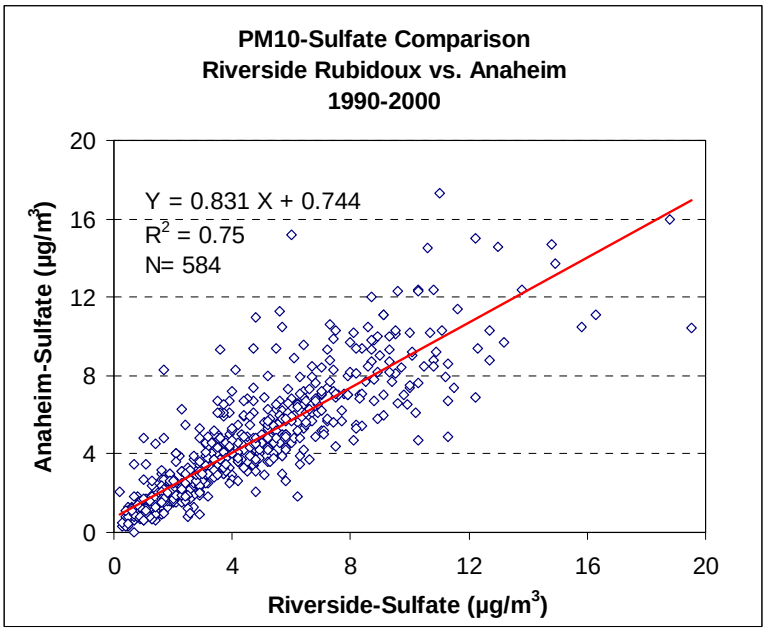
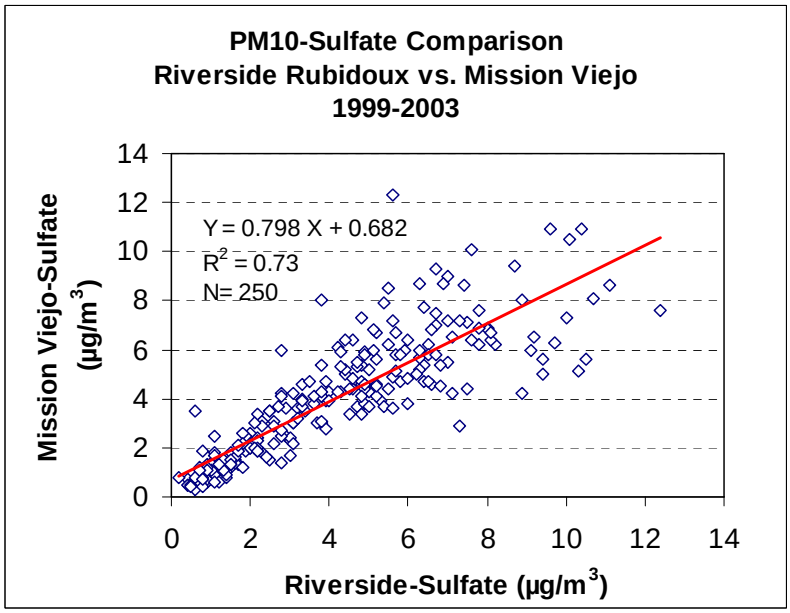


Figure 12. PM10 Sulfate Comparison at Riverside and Mission Viejo.



The observed relatively strong sulfate linkage at sites in Southern California provides quantitative support to the logic for adjusting ambient sulfate data in San Diego. An estimate of "domestic" sulfate in ambient air in the San Diego metropolitan area was constructed by drawing on the similarities of population, land use, and terrain between Orange County (a coastal, less industrial region of South Coast Air Basin) and western San Diego County. Sulfate conversion efficiency (the ratio of SO_X emissions per unit area to average ambient sulfate) was computed from the Orange County SO_X inventory and ambient data from El Toro (Figures 4a-b), then applied to the San Diego County inventory to produce an estimate of the "expected" sulfate in the San Diego metropolitan area due to San Diego County emissions.

e. Summary

Background concentrations are those that would be observed in the absence of anthropogenic emissions of PM and its precursors. Characterizing the background is necessary to determine the exposure and risk associated with regional anthropogenic emission. As emissions continue to be reduced due to the use of cleaner fuel and control technologies, the issue of specifying the background to the exposure of airborne particles has become increasingly important in the regulation of pollutant emissions in the United States.

Stringent regulations on the sulfur content of fuels have minimized sulfur emissions from most California sources, but despite low sulfur content, the large volume of motor fuel used in California still results in significant statewide SO_X emissions, of which goods movement sources such as locomotives, trucks, etc. are a significant fraction. The largest uncontrolled fossil fuel sulfur source in California is the burning of residual oil as fuel in ocean-going vessels.

Sulfate analysis is complicated by the fact that, in addition to sulfate formed from fossil fuel use in California, there are three other sources of atmospheric sulfate in California – natural "background" sulfate formed over the ocean, global "background" sulfate that is distributed throughout the Northern Hemisphere by the upper air westerly winds, and sulfate blown into Southern California from combustion in Mexico.

Natural sulfate concentrations from the ocean were estimated from a review of open ocean measurements and California-specific shore-line and offshore island monitoring data. Sulfate carried by the sea breeze will be reduced by deposition and diluted by dispersion as the air moves inland. Concentrations inland from the shoreline were estimated from the residuals of regressions between SO_X emissions and measured ambient sulfate over the period 1985-2000, and found to agree with expected fall-off going inland. Sulfates in upper air from sources throughout the Northern Hemisphere have been detected at multiple mountain locations in North America, and California-specific data are available from studies in northern California. Since this sulfate is widely distributed over the mid-latitudes, a single upper air "background" level was assigned to all high altitude sites.

Annual average "local" source sulfate at most California monitoring sites was estimated by subtracting site-specific estimated background sulfate (ranged from 0.2 $\mu g/m^3$ to 2 $\mu g/m^3$) from the observed values. In extreme southern California (San Diego and Salton Sea Air Basins), where transport from Mexico adds significantly to the measured

sulfate, additional adjustments were made based on regression analyses and comparison of ambient sulfate concentrations with analogous population centers farther north. The adjustment estimate used in this analysis, is a reasonable “first approximation” and it does not eliminate the need for more detailed study of SO_X transport across the border in southern California.

Examination of the frequency of occurrence of mass concentrations and particle components provides insight not only about annual median conditions but also the variability of apparent background conditions. The results of several data analyses suggest that a more elaborate approach to defining background could improve the present approach.

It should also be noted that the relationship between sulfur oxide emissions and ambient sulfate air quality involves several complex processes. Atmospheric transport and diffusion control the dispersal of the emissions, while chemical oxidation processes lead to the formation of sulfate aerosol from gaseous sulfur dioxide. Spatial distributions of emissions could also be altered by relocation of emission sources, by non-homogeneous growth patterns, and by non-proportional emission changes for different types of sources. Removal processes of SO_2 and particulate sulfate include dry deposition as well as washout by precipitation. Therefore, a thorough and systematic investigation of air quality data and emissions should be performed using a combination of comprehensive air quality and meteorological models. Such an analysis cannot be performed with existing data. Additional emissions data and field work are ongoing by ARB and others to support a process-oriented analysis.

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7. Uncertainty in Exposure Estimates

Secondary nitrate and sulfate particle formation are influenced by a combination of precursor pollutant concentrations and weather conditions. Conversion of SO_X to sulfate aerosols is accelerated by the presence of oxidants in the air (as during ozone episodes) and is greatly accelerated under humid conditions when the conversion can occur inside water droplets. NO_X conversion to nitrate is even more sensitive to weather conditions, as formation rates must compete with dissociation back to gases, so that nitrate is generally a cool-wet (e.g., winter) weather phenomenon. Due to the influences of these factors, the same emissions can result in high PM concentrations on one occasion, and low concentrations on another.

Finally, there is uncertainty in these estimates of the secondary fraction of PM2.5 mass. For example, there was limited ambient speciated data in many areas, particularly rural areas. Additionally, these estimates do not account for the volatilization of NO_3 from the particulate filters during sampling and before analysis. Volatilization could be as high as 50%.

Overall, it seems that our relatively simple methods provide reasonable estimates of the contribution of secondary PM in most of the heavily populated air basins, but the

numbers reported here are not as precise as would be generated by a focused field and modeling program designed around the questions addressed in this study.

8. Discussion of Uncertainty Associated with Data Sources

Measurement Methods

Routine monitoring for sulfate and nitrate in particles utilizes filter sampling and aqueous extraction for ion chromatographic analysis (IC).

This method is highly reliable for sulfate, which, once in particle form is chemically stable. In addition, virtually all sulfur in California PM10 and PM2.5 samples is in the form of sulfate, so that comparison of elemental sulfur analyses with sulfate ion analyses (SO_4= mass = S * 3) provides a “built-in” cross-check on the measurements.

Nitrate IC measurement quality is comparably to that for sulfate, but there are possible sources of sampling error in nitrate data. Particle ammonium nitrate is not chemically stable, but exists in equilibrium with the surrounding air. This equilibrium depends on gaseous concentrations of ammonia and nitric acid, and is also influenced by humidity and temperature. As a typical 24-hour filter sample is collected, these conditions can change, and thus the amount of nitrate on a filter can change, most often as previously collected particle nitrate returns to the gas phase and is lost from the filter, but it is also possible to add artifact nitrate as gas phase precursors react with material on the filter. Standard practice to control for nitrate loss or gain is to place a “denuder” upstream of the filter to remove gas-phase nitric acid from the air stream (preventing positive artifact), and a nylon “backup” filter behind the sampling filter, where volatilized nitric acid will chemically react with the nylon and be collected for measurement.

The sulfate and nitrate measurements from the IMPROVE network are typical of PM10 and PM2.5 filter measurements used in the current study. The IMPROVE Quality Assurance Plan’s (IMPROVE, 2002) measurement objectives for these compounds are listed in the following table.

IMPROVE Measurement Quality Objectives

Method	Parameters	Precision*	Accuracy	MQL
PIXE	Elements S to Mn	±5%	±5%	1 - 4 ng/m3
IC	NO3, SO4, NH4	±5%	±5%	10 - 30 ng/m3

Uncertainty

The 5% uncertainty for an individual sample translates to less than 0.5% in computation of an annual mean for 78 samples (75% completeness in a typical 104 sample-day IMPROVE year). Applying the same measurement quality and completeness criterion to networks reporting only one-in-six day sampling (48 sample minimum) gives an uncertainty just under 1%.

The largest uncertainty in using the annual mean of either sulfate or nitrate comes from inter-annual variability. As an example, IMPROVE annual distributions of SO_4 and NO_3 data at Yosemite for the decade of the 1990s are shown in Figures 13 and 14.

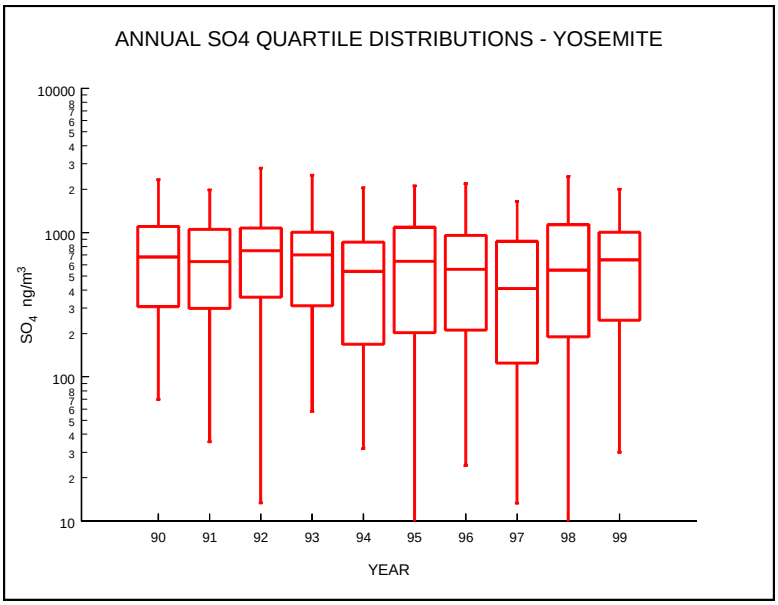

Figure 13.

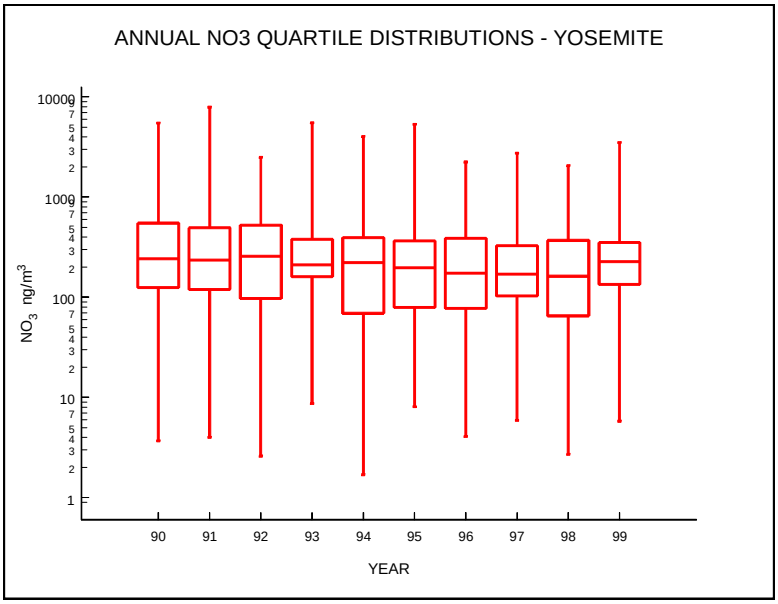

Figure 14.

Since the Yosemite site is not located in an urban area, these data reflect the large impact of meteorology on transport patterns and secondary particle formation. At an urban site, this kind of variation would be added to changes due to local activity around the site.

PM10 and PM2.5 Relative Composition

In order to get the greatest possible spatial coverage of chemically speciated PM data for this analysis, sulfate and nitrate data from both PM10 and PM2.5 measurements were used in the present study. Mixing data from differently size- limited sampling imposes some uncertainty in the analysis. The logic of this is based on the generalization that secondary PM species are concentrated in the fine (<2.5 μm) size fraction, so that collection of PM10, rather than PM2.5, should not significantly change the collected mass of these species. This assumption is supported by paired PM2.5 and PM10 sulfate data from the 1995 PTEP study as shown in the following table.

	San Nicolas Is.	Anaheim	Downtown LA	Diamond Bar	Fontana	Rubidoux	Mainland AVG	Land / Sea
PM 10								
NO3-	1.54	9.4	11.55	11.53	15.52	19.35	11.2	7.3
SO4=	1.96	4.54	5.19	4.24	3.92	4.39	3.7	1.9
PM 2.5								
NO3-	0.68	6	8.47	8.35	11	16.12	8.3	12.2
SO4=	1.4	3.79	4.63	3.88	3.66	3.53	3.2	2.3
RATIO PM2.5 / PM10								
NO3-	0.442	0.638	0.733	0.724	0.709	0.833	0.728	1.6
SO4=	0.714	0.835	0.892	0.915	0.934	0.804	0.876	1.2

These data demonstrate that, except in the immediate vicinity of the ocean, both sulfate and nitrate are preferentially formed in the fine particle phase. This is especially the case for urban sulfate, which averages 87% fine. Note also the reversal of this relationship for NO_3 at San Nicolas Island, indicative of nitric acid reaction within coarse, wet sea salt particles - a sink not available away from the coast.

B. Secondary Organic Aerosols

Introduction

Organic compounds are a significant component of total particulate matter (PM) in the troposphere, so the characterization of sources and composition of organic aerosols is important to our understanding of the potential human health effect of atmospheric PM. Atmospheric particulate carbon consists of both elemental carbon (EC) and organic carbon (OC). Elemental carbon has a chemical structure similar to impure graphite and is emitted directly by sources. Organic carbon can either be emitted directly by sources (primary OC) or can be the result of the condensation of gas-phase oxidation products of volatile organic compounds (VOCs), hereafter referred to as secondary organic aerosol (SOA). Atmospheric carbon particles are emitted from more than 70 different types of air pollution sources (Gray and Cass 1998). Obvious sources include gasoline-powered motor vehicles, heavy-duty diesel vehicles, railroad engines, boilers, aircraft and many other combustors that burn fossil fuel. To the emissions from fuel combustion are added carbon particles from woodsmoke, food cooking operations, and even an ambient concentration increment from such minor sources as cigarette smoke. In addition, there are fugitive sources including the organic carbon content of paved road dust, tire dust and vehicular brake wear particles.

Generally, organic PM concentrations, composition, and formation mechanisms are poorly understood. Particulate organic matter is an aggregate of hundreds of individual compounds spanning a wide range of chemical and thermodynamic properties (Saxena and Hildemann, 1996). Some of the organic compounds are “semivolatile” such that both gaseous and condensed phases exist in equilibrium in the atmosphere. The presence of semivolatile or multiphase organic compounds complicates the sampling process. Understanding the mechanisms of formation of secondary organic PM is important because secondary organic PM can contribute in a significant way to ambient PM levels, especially during photochemical smog episodes.

Ozone and the hydroxyl radical are thought to be the major initiating reactants. Pandis et al. (1992) identified three mechanisms for formation of secondary organic PM: (1) condensation of oxidized end-products of photochemical reactions (e.g., ketones, aldehydes, organic acids, hydroperoxides), (2) adsorption of organic gases onto existing solid particles (e.g., polycyclic aromatic hydrocarbons), and (3) dissolution of soluble gases that can undergo reactions in particles (e.g., aldehydes). The first and third mechanisms are expected to be of major importance during the summertime when photochemistry is at its peak. The second pathway can be driven by diurnal and seasonal temperature and humidity variations at any time of the year. With regard to the first mechanism, Odum et al. (1996) suggested that the products of the photochemical oxidation of reactive organic gases are semivolatile and can partition themselves onto existing organic carbon at concentrations below their saturation concentrations. Thus, the yield of secondary organic PM depends not only on the identity of the precursor organic gas but also on the ambient levels of organic carbon capable of absorbing the oxidation product.

The formation of atmospheric aerosols from biogenic emissions has been of interest for many years. Recent laboratory and field studies support the concept that nonvolatile and semivolatile oxidation products from the photo-oxidation of biogenic hydrocarbons could contribute significantly to ambient PM concentrations in both urban and rural environments. However, further investigations are needed to accurately assess their overall contributions to fine PM concentrations.

Although the mechanisms and pathways for forming inorganic secondary particulate matter are fairly well known, those for forming secondary organic PM are not as well understood. Environmental chamber experiments, molecular chemical analyses kinetic and mechanistic studies of chemical reactions, and detailed modeling have been conducted to improve the understanding of the SOA formation and its contribution to fine PM (Schauer and Cass 1998; Griffin et al, 2002; Schauer et al, 2002; Pandis et al, 2003). These studies showed that SOA can be between 15 and 60 percent of the total OC. They also indicated that SOA formation processes are complex and even the most detailed models have substantial uncertainties. Understanding such processes poses a number of experimental and theoretical challenges, because of the large number of condensable products in SOA, the structural complexity of such components, and the importance of aerosol formation.

Estimate of SOA using Source OC/EC Ratios

No analytical method by itself is able to distinguish between primary and secondary organic material. Additional information or assumptions have to be used to make an estimate of the relative contributions of primary and secondary organic compounds to the ambient aerosol. Secondary organic carbon can be estimated by a relatively simple empirical method, if the ratio of organic carbon (OC) to elemental carbon (EC) of the major primary emissions is known (Turpin and Huntzicker, 1990, 1991a,b, 1995). EC is often used as a tracer of primary anthropogenic emissions, and is inert in the atmosphere. The secondary organic carbon in the ambient aerosol can then be estimated as the excessive organic carbon, which cannot be explained by common origin with the elemental carbon according to primary OC/EC ratios. This secondary organic carbon is given as:

$$OC_{secondary} = OC_{total} - OC_{primary} ,$$

$$OC_{primary} = EC*(OC/EC)_{primary}$$

where $OC_{secondary}$ is the secondary organic carbon, $OC_{primary}$ the primary organic carbon, OC_{total} the total measured organic carbon, and $(OC/EC)_{primary}$ the estimate of the primary OC/EC ratio. One of the major uncertainties of this method is the estimate of the (OC/EC) ratio for primary emissions. The OC/EC ratios are strongly source dependent and therefore quite variable. In practice $(OC/EC)_{primary}$ is defined as the ambient OC/EC ratio at times when the formation of SOA is supposed to be negligible. This is the case on days that are characterized by lack of direct sunlight, low ozone concentrations and an unstable air mass. The estimates for $(OC/EC)_{primary}$ lie between 1.7 and 2.9 (Turpin

et al. 1995). Lurmann et al. (2005) used the OC/EC method to determine the contribution of secondary organic aerosol. The ratio of OC/EC representing primary emissions was calculated to be 2.74 based on 2000/2001 CRPAQS data from Bethel Island, Bakersfield, Fresno, Angiola, and Sierra Nevada Foothills when the OC/EC ratio was less than 3.5.

The initial PM analysis for goods movement only addressed primary carbonaceous material. To complete the assessment of goods movement, PM effects from the contribution to SOA must also be obtained. Because direct measurements of SOA are not available in routine PM data, the ratio of OC to EC can be used to estimate the amount of SOA in a given sample. The ambient EC and OC data used in this analysis were obtained from the California Regional PM10/PM2.5 Air Quality Study (CRPAQS), IMPROVE, and CHS monitoring programs. The OC/EC method was used to determine the contribution of SOA at PM monitoring sites in California in 2000. Using this ratio, the contribution of SOA at about 50 sites in California range from 0.15 $\mu g/m^3$ to 2.40 $\mu g/m^3$ (Figures 15a-b).

Population-weighted SOA exposure was computed by estimating local SOA concentrations at the census block level using spatial interpolation of the monitoring data. Finally, aggregated air basin health effects were estimated from the population-exposure data and the fraction of those effects due to goods movement emissions determined based on local emission inventories. The effects of the uncontrolled ship emissions on port-area air quality show up in these calculations: roughly less than 1 percent of the health effects due to international goods movement (*i.e.* shipping and port operations) are due to SOA.

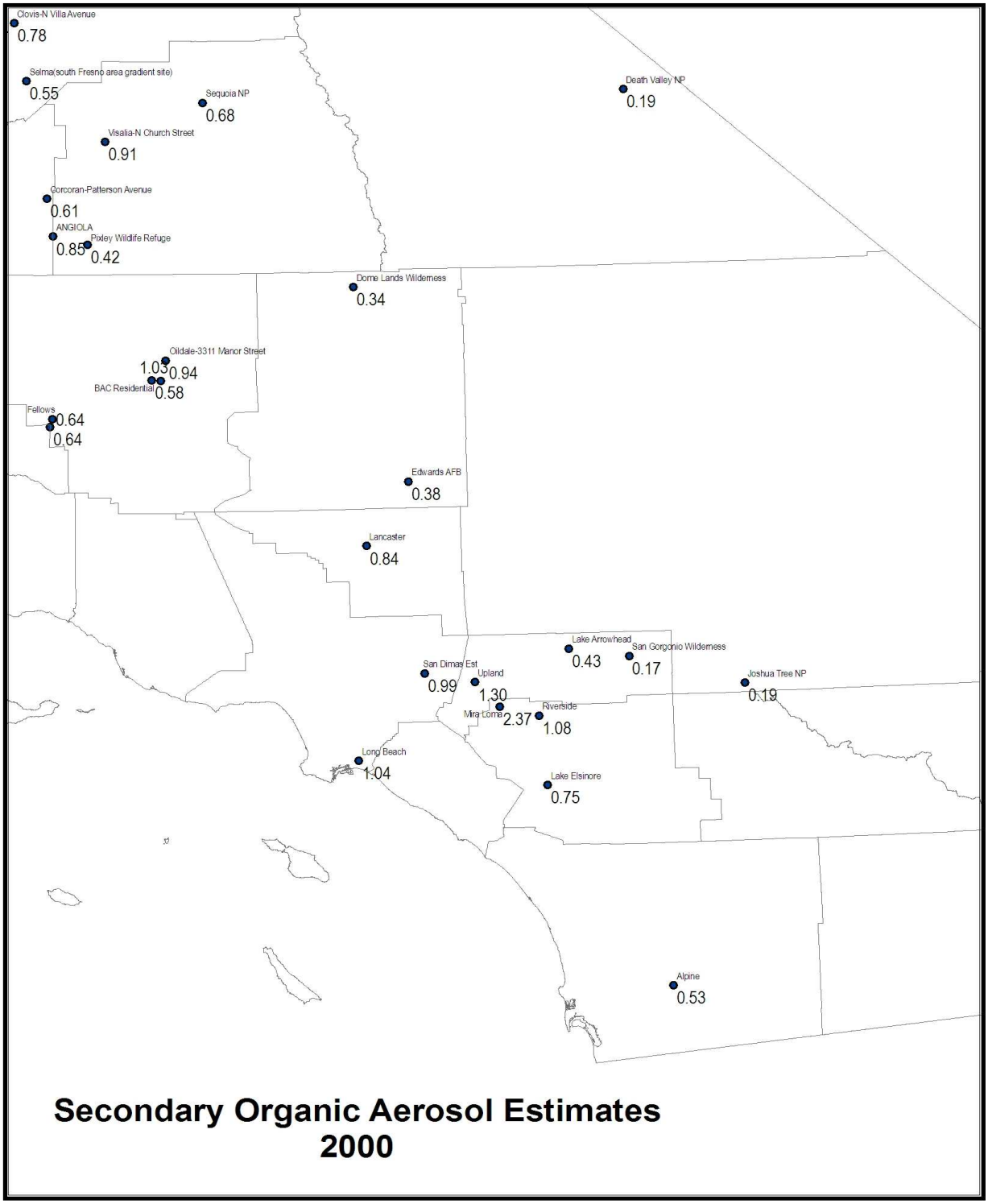

Figure 15a. Estimated SOA concentrations at monitoring sites in Southern California.

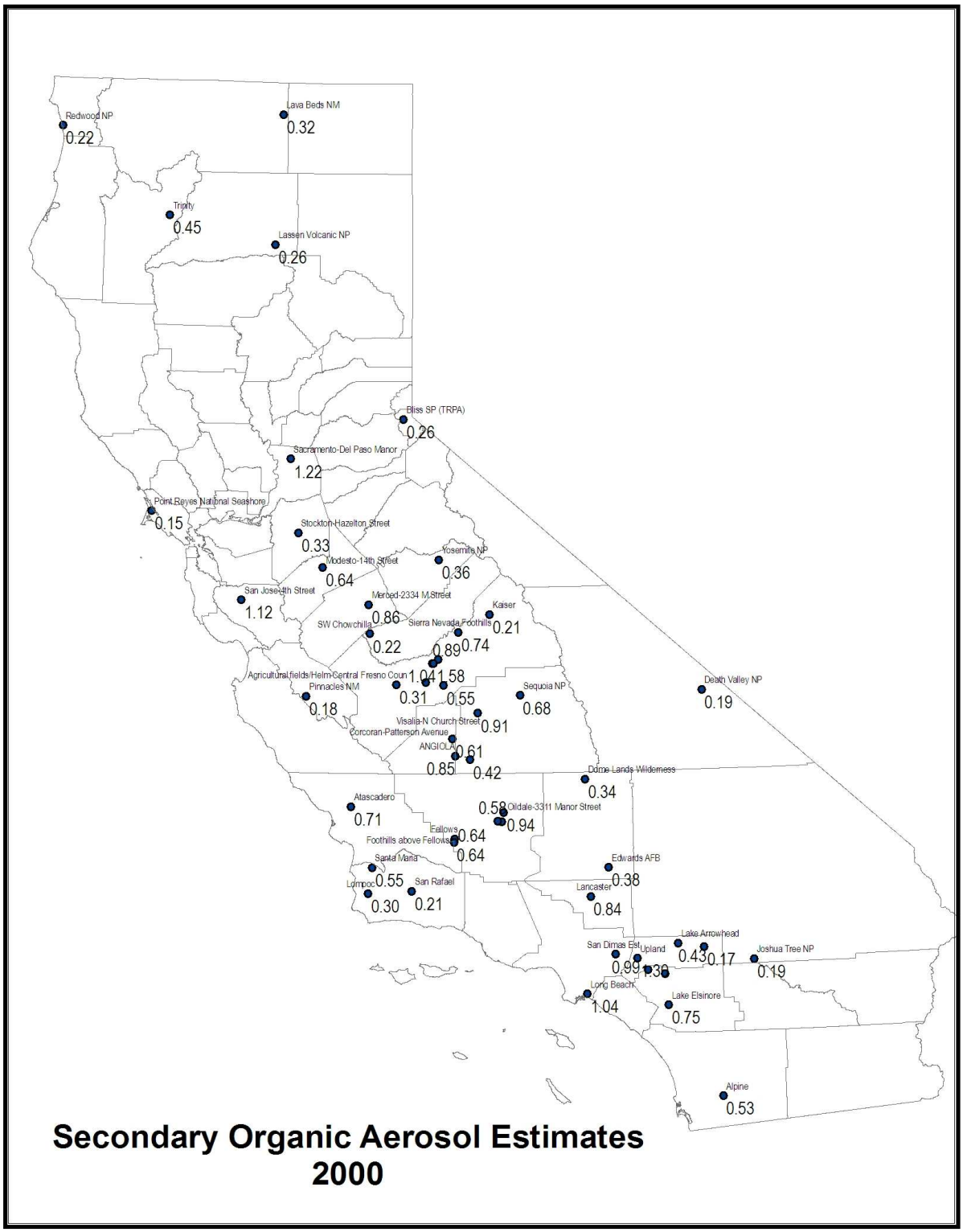

Figure 15b. Estimated SOA concentrations at PM monitoring sites in California.

Uncertainty

A high degree of uncertainty is associated with all aspects of the calculation of secondary organic PM concentrations. Currently, it is not possible to fully quantify the concentration, composition, or sources of the organic components. Many of the secondary organic aerosol components are highly oxidized, difficult to measure, multifunctional compounds. This is compounded by the volatilization of organic carbon from filter substrates during and after sampling as well as potential positive artifact formation from the absorption of gaseous hydrocarbon on quartz filters. In addition, no single analytical technique is currently capable of analyzing the entire range of organic compounds present in the atmosphere in PM. Even rigorous analytical methods are able to identify only 10 to 20% of the organic PM mass on the molecular level (Rogge et al. 1993a; Schauer et al. 1996).

Environmental smog chambers can be useful in elucidating the chemical mechanisms associated with the formation of compounds found in organic PM; however, significant uncertainties always arise in the interpretation of smog chamber data because of wall reactions. Limitations also exist in extrapolating the results of smog chamber studies to ambient conditions found in urban airsheds. Additional laboratory studies are needed to comprehensively identify organic compounds, strategies need to be developed to sample and measure such compounds in the atmosphere, and models of secondary organic aerosol formation need to be improved and added to air quality models in order to address compliance issues related to reducing PM mass concentrations that affect human exposure.

Since primary OC and EC are mostly emitted from the same sources, EC can be used as a tracer for primary combustion-generated OC (Gray et al., 1986; Turpin and Huntzicker, 1995; Strader et al., 1999). The formation of SOA increases the ambient concentration of OC and the ambient OC/EC ratio. OC/EC ratios exceeding the expected primary emission ratio are an indication of SOA formation. Primary ratios of OC and EC vary from source to source and show temporal and diurnal patterns, but since EC is only emitted from combustion sources, gaseous tracers of combustion (CO, NO, NOx) can be used to determine periods dominated by primary aerosol emissions (Cabada et al., 2004). Ozone is an indicator of photochemical activity, and it also can be used as a tracer for periods where secondary organic aerosol production is expected. In this case, increases in the OC/EC ratio correlated to ozone episodes are indicative of SOA production.

The major weakness of the method is its reliance on the assumption of a constant primary OC/EC during the analysis period. Variations of sources strengths, meteorology, etc. are expected to change the primary OC/EC. This assumption can be relaxed if there are high temporal resolution data by grouping the data by period of day, month, etc. In any case, this variability introduces significant uncertainties in the estimated SOA concentration. Even if such an almost constant ratio exists, its

determination is non-trivial. The primary OC/EC ratio is primarily determined either from measurements during periods where the primary sources dominate the ambient OC or from emission inventories (Gray, 1986; Cabada et al., 2002).

Estimates of secondary organic aerosol formation based on changes in ambient OC/BC are uncertain because the proportion of vehicles that are diesel-powered and driving at a given hour varies throughout the day. Source tests have shown that the black carbon fraction of total fine carbonaceous particle emissions is higher in diesel exhaust than it is in gasoline exhaust. Hildemann et al. (1991) report that black carbon accounted for 11 and 33% of fine carbonaceous particle emissions from non-catalyst and catalyst-equipped gasoline-powered vehicles, respectively, versus 55% black carbon in diesel engine carbon particle emissions. Likewise Watson et al.(1994) report a lower black carbon fraction, 31% of total fine carbon particles, in gasoline engine exhaust compared to 45% black carbon in diesel engine exhaust emissions. DOE's gasoline/diesel PM split study (Fujita et al, 2005 http://www.nrel.gov/vehiclesandfuels/nfti/feat_split_study.html) was conducted to assess the sources of uncertainties in quantifying the relative contributions of tailpipe emissions from gasoline powered and diesel-powered motor vehicles to the ambient concentrations of fine particulate matter (PM2.5). This study indicates that other important contributors to the variation of the OC/EC ratio are: the specific driving cycle (road type, traffic conditions), fleet composition, percentage of high emitters, and percentage of visibly smoking vehicles. Therefore, differences in the diurnal patterns of light- and heavy-duty vehicle activity may also contribute to variations in the ambient OC/EC ratio.

A critical issue here is the categorization of volatile organic compounds (VOC) emissions, and how that relates to formation of SOA. Many different types of VOCs are emitted into the atmosphere, where they can affect SOA formation at different rates. One of the major uncertainties is the assumption of all ROG emissions have equal propensity to form SOA. Diesel emissions are supposed to contain a high fraction of high molecular weight compounds (especially from ships), which could also influence SOA production.

Currently, the details of SOA formation are not well known and the implications for needs related to the development of emission factors and other emissions estimation tools to characterize the precursor emissions are uncertain. Large carbon number organic compounds that have an affinity to stick together could contribute significantly to these processes. Future development efforts may need to be directed to expand VOC speciation profiles to include compounds that improve the methods for characterizing SOA formation. Additional uncertainties are associated with lack of proper time and spatial resolution in ambient measurements of both primary and secondary organic species. These detailed measurements are critical in evaluating influence of meteorology and diurnal and seasonal changes in emissions. Measurements of the SOA product concentrations could be helpful in estimating the SOA concentrations

using observations. However, one needs to be careful because many of these species continue reacting in the atmosphere and therefore are not conserved as SOA tracers.

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C. Calculation Protocol

Below, we provide the SAS program used to calculate health impacts associated with exposures to DPM. Similar programs for calculating health impacts associated with exposures to nitrates, secondary organic aerosols, other primary PM2.5, or ozone are also available. Contact Hien Tran (htran@arb.ca.gov) for additional information.

```
/* goods mvt plan: INTERNATIONAL GM
   primary diesel pm using ab info (Pope 2002 for death)
   file effect_dpm_v10_030306.sas   03/03/06, h.t  */

libname gmp 'C:\My Documents\ARB\GoodsMovtPlan\PrimaryDPM';
libname gm 'C:\My Documents\ARB\GoodsMovtPlan';
libname gma 'C:\My Documents\ARB\GoodsMovtPlan\Abt';


/* STEP 1: get emissions data & health effects to calculate factors */

/* import health effects:  */

data effect1;
set gma.impacts_ab_dpm_sulfates_nitrates;
if poln='DirectPM';
drop poln;
rename mean=effect_mean lower=effect_lower upper=effect_upper;
run;

/* import all emissions
/* calculate ab emissions */
data allems;
set gm.gm_all_ems_24nm_adj_030306;
run;

proc sort data=allems out=temp100;
by ab poln;
run;
proc univariate data=temp100 noprint;
by ab poln;
var ems2000 ems2005 ems2010 ems2015 ems2020 ems2025;
output out=allems_ab sum=allems2000 allems2005 allems2010 allems2015
allems2020 allems2025;
run;

/* STEP 2: calculate factors */

/* get ab factors for health effect 1: mortality */
data allems_ab_dpm;
set allems_ab;
if poln='DPM';
run;
proc sort data=effect1 out=temp101;
by ab;
run;
data temp102;
set temp101;
```

```
if ab='SFB' then ab='SF';
run;
data temp103;
merge temp102 allems_ab_dpm;
by ab;
run;

data factors1;
set temp103;
factor1_lower=allems2000*365/effect_lower;
factor1_mean=allems2000*365/effect_mean;
factor1_upper=allems2000*365/effect_upper;
drop allems2005 allems2010 allems2015 allems2020 allems2025;
run;

/* STEP 3: get population for each year */

PROC IMPORT OUT= WORK.pop_all
            DATAFILE= "C:\My Documents\ARB\GoodsMovtPlan\pop by coabdis 1995-
2050.dbf"
            DBMS=DBF REPLACE;
     GETDELETED=NO;
RUN;

/* calculate ab populations */
proc sort data=pop_all out=temp120;
by ab ;
run;
proc univariate data=temp120 noprint;
by ab ;
var p2000 p2005 p2010 p2015 p2020 p2025;
output out=pop_ab sum=p2000 p2005 p2010 p2015 p2020 p2025;
run;

/* STEP 4: get emissions due to GM */

data gmems;
set gm.gm_ems_int_adj_030306; /* internatioal GM ems */
run;

/* calculate ab emissions */
proc sort data=gmems out=temp110;
by ab co type poln;  /* county */
run;
proc univariate data=temp110 noprint;
by ab co type poln;   /* county */
var ems2000 ems2005 ems2010 ems2015 ems2020 ems2025;
output out=gmems_ab sum=ems2000 ems2005 ems2010 ems2015 ems2020 ems2025;
run;

data gmems_ab_dpm;
set gmems_ab;
if poln='DPM';
run;

/* merge GM emissions w/ populations and factors */
```

```
proc sort data=factors1 out=temp400; by ab; run;
proc sort data=gmems_ab_dpm out=temp410; by ab; run;
proc sort data=pop_ab out=temp420; by ab; run;

data combine_1;
merge temp400 temp410 temp420;
by ab;
run;
/* combine factors w/ gm ems and pop for each endpoint separately */

data factors11;
set factors1;
if endpoint='1_mortality_pope';
run;
data combine_11;
merge factors11 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors12;
set factors1;
if endpoint='2_hosp_resp';
run;
data combine_12;
merge factors12 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors13;
set factors1;
if endpoint='3_hosp_cardio';
run;
data combine_13;
merge factors13 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors14;
set factors1;
if endpoint='4_LRS';
run;
data combine_14;
merge factors14 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors15;
set factors1;
if endpoint='4b_asthma_exac';
run;
data combine_15;
merge factors15 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors16;
set factors1;
```

```
if endpoint='5_acute_bronc';
run;
data combine_16;
merge factors16 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors17;
set factors1;
if endpoint='6_wld';
run;
data combine_17;
merge factors17 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors18;
set factors1;
if endpoint='7_mrad';
run;
data combine_18;
merge factors18 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors19;
set factors1;
if endpoint='chronic_phlegm';
run;
data combine_19;
merge factors19 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors110;
set factors1;
if endpoint='ervisits_resp';
run;
data combine_110;
merge factors110 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors111;
set factors1;
if endpoint='hosp_resp_linn';
run;
data combine_111;
merge factors111 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors112;
set factors1;
if endpoint='mortality_infant';
run;
data combine_112;
```

```
merge factors112 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors113;
set factors1;
if endpoint='mortality_jerrett';
run;
data combine_113;
merge factors113 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors114;
set factors1;
if endpoint='mortality_j+p';
run;
data combine_114;
merge factors114 gmems_ab_dpm pop_ab ;
by ab;
run;

data factors115;
set factors1;
if endpoint='mortality_p+j';
run;
data combine_115;
merge factors115 gmems_ab_dpm pop_ab ;
by ab;
run;
/* combine all */
data combine_all;
set combine_11 combine_12 combine_13 combine_14 combine_15 combine_16
combine_17
combine_18 combine_19 combine_110 combine_111 combine_112 combine_113
combine_114
combine_115;
run;

/* add 'year' variable */
data combine_1a; set combine_all; year=2005;
data combine_1b; set combine_all; year=2010;
data combine_1c; set combine_all; year=2015;
data combine_1d; set combine_all; year=2020;
data combine_2;
set combine_1a combine_1b combine_1c combine_1d;
run;

/* STEP 5: calculate GM health impacts by category */

data effect_ab_test;
set combine_2;
if year=2005 then do;
gm_effect=(ems2005*365/factor1_mean)*(p2005/p2000);
gm_effect_lower=(ems2005*365/factor1_lower)*(p2005/p2000);
gm_effect_upper=(ems2005*365/factor1_upper)*(p2005/p2000);
end;
```

```
else if year=2010 then do;
gm_effect=(ems2010*365/factor1_mean)*(p2010/p2000);
gm_effect_lower=(ems2010*365/factor1_lower)*(p2010/p2000);
gm_effect_upper=(ems2010*365/factor1_upper)*(p2010/p2000);
end;
else if year=2015 then do;
gm_effect=(ems2015*365/factor1_mean)*(p2015/p2000);
gm_effect_lower=(ems2015*365/factor1_lower)*(p2015/p2000);
gm_effect_upper=(ems2015*365/factor1_upper)*(p2015/p2000);
end;
else if year=2020 then do;
gm_effect=(ems2020*365/factor1_mean)*(p2020/p2000);
gm_effect_lower=(ems2020*365/factor1_lower)*(p2020/p2000);
gm_effect_upper=(ems2020*365/factor1_upper)*(p2020/p2000);
end;
run;

/* sum by category by ab */

proc sort data=effect_ab_test out=temp500;
by year endpoint ab type;
run;
proc univariate data=temp500 noprint;
var gm_effect_lower gm_effect gm_effect_upper;
by year endpoint ab type;
output out=gm_effect_type_ab sum=gm_effect_lower gm_effect gm_effect_upper;
run;

/* sum across categories for each ab */
proc sort data=effect_ab_test out=temp510;
by year endpoint ab;
run;
proc univariate data=temp510 noprint;
var gm_effect_lower gm_effect gm_effect_upper;
by year endpoint ab;
output out=gm_effect_ab sum=gm_effect_lower gm_effect gm_effect_upper;
run;

/* sum across basins for statewide totals */
proc univariate data=gm_effect_ab noprint;
var gm_effect_lower gm_effect gm_effect_upper;
by year endpoint;
output out=gm_effect_sw sum=gm_effect_lower gm_effect gm_effect_upper;
run;

/* STEP 6:  export files */
PROC EXPORT DATA= gm_effect_type_ab
            OUTFILE= "C:\My
Documents\ARB\GoodsMovtPlan\PrimaryDPM\gm_int_dpm.xls"
            DBMS=EXCEL REPLACE;
     SHEET="type_ab";
RUN;

PROC EXPORT DATA= gm_effect_ab
            OUTFILE= "C:\My
Documents\ARB\GoodsMovtPlan\PrimaryDPM\gm_int_dpm.xls"
            DBMS=EXCEL REPLACE;
```

```
     SHEET="ab";
RUN;
PROC EXPORT DATA= gm_effect_sw
            OUTFILE= "C:\My
Documents\ARB\GoodsMovtPlan\PrimaryDPM\gm_int_dpm.xls"
            DBMS=EXCEL REPLACE;
SHEET="statewide";
RUN;

PROC EXPORT DATA= factors1
            OUTFILE= "C:\My
Documents\ARB\GoodsMovtPlan\PrimaryDPM\gm_int_dpm.xls"
            DBMS=EXCEL REPLACE;
SHEET="factors";
RUN;

/*  PART 5: FINAL OUTPUTS  */

/* One composite table */

data results_all;
set gm_effect_type_ab(rename=(gm_effect=mean gm_effect_lower=lower
gm_effect_upper=upper));
poln='DPM';
run;

PROC EXPORT DATA= results_all
            OUTFILE= "C:\My
Documents\ARB\GoodsMovtPlan\PrimaryDPM\gm_int_dpm_all.xls"
            DBMS=EXCEL REPLACE;
            SHEET="gm_int_dpm";
RUN;
data gmp.gm_int_dpm_all; set results_all; run;

/*  PART 4b: regional sums, ab1 is a region */

data effect_ab1; set effect_ab_test;
if ab='SC' then ab1='1_SC    ';
else if ab='SF' then ab1='2_SFB   ';
else if ab='SD' then ab1='3_SD    ';
else if ab='SJV' then ab1='4_SJV   ';
else if ab='MC' and co in (9,31) or ab='SV' and co in (31,34,48,57)  then
ab1='5_SacFNA   ';
else ab1='6_Others   ';
run;

/* sum across categories for each ab */
proc sort data=effect_ab1 out=temp510;
by year endpoint ab1;
run;
proc univariate data=temp510 noprint;
var gm_effect_lower gm_effect gm_effect_upper;
by year endpoint ab1;
output out=gm_effect_ab1 sum=gm_effect_lower gm_effect gm_effect_upper;
run;

PROC EXPORT DATA= gm_effect_ab1
```

```
            OUTFILE= "C:\My
Documents\ARB\GoodsMovtPlan\PrimaryDPM\gm_int_dpm.xls"
            DBMS=EXCEL REPLACE;
     SHEET="region";
RUN;

/* sum across categories for each type, each region */
proc sort data=effect_ab1 out=temp510;
by year endpoint type ab1;
run;
proc univariate data=temp510 noprint;
var gm_effect_lower gm_effect gm_effect_upper;
by year endpoint type ab1;
output out=gm_effect_type_ab1 sum=gm_effect_lower gm_effect gm_effect_upper;
run;

data results_all1;
set gm_effect_type_ab1(rename=(gm_effect=mean gm_effect_lower=lower
gm_effect_upper=upper));
poln='DPM';
run;

data gmp.gm_int_dpm_region; set results_all1; run;

/* end of 3/13/06 h.t  */
```

The next table shows the basin-specific factors (tons per case of health endpoint) used in calculating the health impacts. PM2.5 impacts were based on DPM factors. Details on how these factors are used can be found in the Health Impacts Methodology Section of Appendix A.

Air Basin	Pollutant	Death	Hospitalization (respiratory)	Hospitalization (cardiovascular)	Asthma and Other Lower Respiratory Symptoms	Acute Bronchitis	Work Loss Days	Minor Restricted Activity Days
GBV	DPM	193	1021	550	11	131	1.68	0.28
LC	DPM	59	360	202	5	57	0.85	0.14
LT	DPM	67	308	163	3	32	0.46	0.08
MC	DPM	44	208	112	2	29	0.39	0.07
MD	DPM	86	437	236	3	31	0.50	0.09
NC	DPM	90	514	279	5	62	0.80	0.13
NCC	DPM	30	136	74	1	12	0.16	0.03
NEP	DPM	71	387	211	4	52	0.65	0.11
SC	DPM	6	30	16	0	3	0.04	0.01
SCC	DPM	21	92	50	1	9	0.13	0.02
SD	DPM	12	54	30	0	6	0.07	0.01
SF	DPM	13	58	32	1	7	0.07	0.01
SJV	DPM	28	148	81	1	11	0.19	0.03
SS	DPM	16	78	43	1	7	0.11	0.02
SV	DPM	20	106	58	1	11	0.15	0.03
GBV	NO_X	2,320	12,259	6,646	136	1,634	21.12	3.56
LC	NO_X	816	4,949	2,770	66	790	11.65	1.95
LT	NO_X	3,654	16,809	8,885	145	1,735	25.25	4.27
MC	NO_X	1,134	5,340	2,882	63	761	10.03	1.68
MD	NO_X	980	4,994	2,682	30	360	5.70	0.99

NC	NO_X	2,751	15,530	8,430	158	1,885	24.55	4.14
NCC	NO_X	1,733	7,927	4,288	61	726	9.56	1.65
NEP	NO_X	6,629	36,206	19,741	404	4,829	60.65	10.27
SC	NO_X	193	905	491	6	79	1.10	0.19
SCC	NO_X	777	3,482	1,902	29	353	4.82	0.83
SD	NO_X	317	1,459	804	13	152	1.90	0.33
SF	NO_X	1,034	4,710	2,546	44	530	5.93	1.03
SJV	NO_X	1,022	5,442	2,967	33	403	6.86	1.19
SS	NO_X	538	2,671	1,484	19	234	3.92	0.68
SV	NO_X	973	5,090	2,783	42	498	7.10	1.22
GBV	ROG	9,657	51,291	27,742	563	6,727	86.84	14.64
LC	ROG	2,007	12,185	6,820	162	1,937	28.69	4.79
LT	ROG	5,413	24,909	13,163	215	2,570	37.41	6.33
MC	ROG	2,928	13,688	7,375	155	1,862	25.04	4.20
MD	ROG	1,887	9,498	5,121	58	693	10.96	1.90
NC	ROG	4,359	24,897	13,506	254	3,035	39.25	6.62
NCC	ROG	2,069	9,539	5,157	74	885	11.51	1.98
NEP	ROG	9,354	51,110	27,895	576	6,891	87.12	14.74
SC	ROG	746	3,501	1,900	24	292	4.21	0.73
SCC	ROG	1,671	7,532	4,104	62	744	10.35	1.77
SD	ROG	1,309	6,052	3,339	51	612	7.87	1.36
SF	ROG	1,152	5,203	2,810	48	572	6.45	1.12
SJV	ROG	2,001	10,619	5,793	64	767	13.33	2.30
SS	ROG	3,521	17,348	9,584	117	1,398	24.14	4.17
SV	ROG	960	5,019	2,742	41	489	6.98	1.20

D. Scientific Peer Review Comments After 12/1/2005 and CARB Staff Responses

When the draft plan was released in December 2005, the plan was submitted for peer review to ten nationally known experts in emissions inventory development, air quality and exposure, health impacts quantification, and economic valuation. These experts include:

- Professor John Froines (UC Los Angeles),
- Professor Jane Hall (CSU Fullerton),
- Aaron Hallberg (Abt Associates, Inc.),
- Professor Michael Jerrett (University of Southern California),
- Dr. Melanie Marty and Dr. Bart Ostro (Office of Environmental Health Hazard Assessment),
- Professor Constantinos Sioutas (University of Southern California), and
- Professor Akula Venkatram (UC Riverside).

Professor James Corbett (University of Delaware) and Professor Robert Harley (UC Berkeley) commented on the emission inventory; their comments and staff responses are presented in a separate supplement on emissions. Dr. Bart Ostro's comments were considered to be internal, as he works closely with CARB staff on ambient air quality standard setting and health impacts quantification. Hence, his comments were incorporated but not formally addressed in this supplement. Comments from the peer reviewers and CARB's responses to those comments are provided in this section. In many cases, we revised our approach in response to their suggestions.

1. Professor John Froines, University of California, Los Angeles

The purpose of this submission is to provide comments on the ARB document entitled Quantification of the health and economic impacts of air pollution from ports and international goods movement in California. In preparing these comments, I have discussed the document and issues with Dr. Dale Hattis, Clark University, Dr. Beate Ritz, UCLA, Dr. Michael Jerrett, University of Southern California, and Dr. Arthur Winer, UCLA. I have attached Dr. Jerrett's comments as an appendix to my comments, since they represent my views as well and should be considered as such. I have incorporated my discussions with Drs. Ritz and Hattis in this document.

At the outset I want to state unequivocally that I have the highest regard for the authors of Appendix A. I think they have done excellent work under very difficult time constraints. I consider their efforts to be a credit to ARB and its management.

I consider it crucial to look at Appendix A in the context of the entire process that is underway. The expansion of Goods Movement in California and the implications for the growth of the Transportation Sector are far reaching. Expansion of goods

movement as a key element of the transportation sector will 1) impact the U.S. and California's commitment to economic globalization; 2) have implications for global climate change; 3) have a dramatic effect on the economy including restructuring of the workforce, capital investment, and introduction of new technologies in the State; 4) affect our relationships with other trading partners, in particular, with Central and South American and Asian countries; 5) have an impact on the regulatory environment for protecting human health and the environment; and finally, 6) affect the quality of people's lives in the State.

Given this context of the very broad implications of goods movement on social policy decision-making we need to address matters of health as completely as possible. Overall, I consider the document to inadequately address the health issues that should be considered if the consequences of these important decisions are to be understood especially if this is the only document that will address health. There is a danger that we are missing the proverbial forest by focusing on the issues so narrowly. Since major societal changes that affect the entire population are being considered, the analysis should address the issues more broadly. The question is not simply one of a three-fold increase in goods movement; the issue has more to do with the overall commitment to a new direction of the economy, that is, the commitment to the transportation sector representing a focal point of the State's economy. I recognize that the mandate of ARB is narrower than the overall issue, and that may require involvement of other Agencies, e.g., CAL/EPA, Department of Industrial Relations and the Department of Health Services, but ultimately the health and social consequences must be evaluated more fully.

CARB Staff Response: Staff recognizes the implications of our analysis results and attempts to explicitly state sources of uncertainties in our report. As California's air quality management agency, our analysis focuses on outdoor air pollution.

The Executive Summary states in the "Uncertainties" section:

"There are significant uncertainties involved in quantitatively estimating the health effects of exposure to outdoor air pollution. … It was not possible to quantify all possible health benefits that could be associated with reducing port-related goods movement emissions".

An attempt is required that seeks to quantify more of the uncertainties. It is possible to more fully quantify the issues and it is possible to estimate the significance of certain endpoints in a qualitative context. It is essential to make some estimates of the consequences of the decisions even if the data are limited rather than to throw up one's hands and say we cannot cope with the uncertainties. That is not an adequate approach to such a complex set of issues. The following considerations seem relevant:

1. **Health impacts:** Health impacts are not limited to outdoor air pollution. In addition to air pollution effects these include at a minimum, psychosocial factors (stress), noise (including cardiovascular effects), light and its effects on sleep, major occupational issues including workplace exposures and injuries, traffic accidents and associated morbidity/mortality, other transportation related issues, and environmental consequences, the latter apparently poorly defined to date.

CARB Staff Response: As California's air quality management agency, our analysis focuses on outdoor air pollution. Other agencies have expertise and responsibilities in these other areas.

2. **Quality of life/disability/morbidity over long periods of time and relation to health care costs:** It is important to recognize that health impacts may occur throughout one's life with associated costs, morbidity, and disability. This issue is not adequately addressed. I would refer you to the work of a number of investigators on the notion of quality-adjusted life year (QALY) and disability life year (DALY) which has been used as a unit to quantify utility of health for policy decision-making (Gold et al, 1996, Patrick and Erickson, 1993, Weinstein and Stason, 1993, and the December, 2005 issue of the American Journal of Industrial Medicine as a reference source). For example, exposures in utero, in the postnatal period, and early in life when development is underway are periods of particular vulnerability and may result in health consequences which will be manifested throughout life. Early development of chronic disease as a result of air pollution exposures may produce morbidity and health care costs over an extended period of time. There may be enormous health care costs, impaired function, and a range of health problems associated with long term morbidity that should be addressed. For example, persons with early development of asthma and atherosclerosis associated with air pollution at a relatively early age will be impacted over a long period of time and these factors need to be recognized and addressed even if the quantitative data are limited.

   CARB Staff Response: Staff has addressed these types of measures through consideration of health endpoints such as minor restricted activity days, work loss days, lower respiratory symptoms. Other morbidity endpoints are also included in a sensitivity analysis.

3. **Quantitative estimates of health outcomes:** Research in the past decade has demonstrated a wide range of health endpoints previously not understood. This is not dissimilar to the growth of our knowledge on environmental tobacco smoke where there were dramatic changes between the first ARB document in 1997 and 2006. There is no reason that quantitative estimates of other health outcomes from air pollution cannot be made. For example, Dr. Ritz has commented to me regarding her work as follows: "About the port estimates, you are completely correct that there is no reason whatsoever to just look at mortality for particles. You can easily expand any risk assessment calculations to include other outcomes; it is in principle the same stuff just using different sets of numbers such as the % exposed at certain levels and the risk ratio for the outcome at that level of exposure in the population and you can calculate the attributable fraction in the exposed or in the population and a number of cases to go with it, and then you attach a $ value to those (due to treatment or lost wages or lost life years etc). There is absolutely no reason to ignore an outcome if risk ratios have been provided by epidemiologic studies and you know the population exposure distribution." Quantitative data are available for a wide range of health endpoints even if they represent surrogates or are indirect. There is no reason not to use them; this is especially true for developmental, cardiovascular and respiratory effects including the Children's Health Study, Dr. Beate Ritz's developmental work and a wide range of recent work on cardiovascular endpoints, e.g., Devlin's trooper study; Kunzli's EHP,

2005, Dockery et al, EHP, 2005; and Henneberger et al, EHP, 2005. These represent only a few examples of highly relevant work to make the point. I also refer you to the 2006 paper by Wang and Mauzerall. There are probably a hundred other studies that could be used to more fully address the wide range of endpoints associated with air pollution health outcomes. These studies should not be addressed in the form of a literature review, but rather what are the quantitative and qualitative implications from their findings in terms of goods movement.

CARB Staff Response: Staff has done an extensive review of the latest epidemiological literature and selected health endpoints that carry a strong "weight of evidence" for a causal association between air pollution and health. Acute bronchitis and chronic phlegm among asthmatics are included in a sensitivity analysis. Low birth weight does not satisfy the criteria for inclusion in our quantitative evaluation at this time.

4. **Reliance on control strategies, regulations, voluntary action and new technologies:** There are major assumptions in the document about the implementation of State rules, Federal rules, incentives, voluntary measures, innovative strategies, engine replacements, land use decisions, efficiency improvements, cleaner fuels and new technology. While I welcome all these approaches and innovations I also believe one has to be realistic about compliance and the implementation of these approaches. The key policy question is what happens if the pace is slowed or even almost non-existent or other factors emerge that result in increased pollution.

   There is a need to determine policy alternatives with respect to the worst case scenarios rather than assuming the best possible case. In other words the policy maker has to address upper bound of risk in terms of decision making and policy formulation as well as assuming effective controls. In my experience in the regulatory world, it is apparent that compliance always and I mean always occurs more slowly than anticipated. This is likely to be especially true where diesel engines are concerned because of their anticipated long life. For example, on page ES-11 of the overall document it is apparent that staff estimates that the diesel reduction targets will not be achieved as the ARB had hoped.

   Research conducted over the past 7 years in the LA Basin clearly illustrate that the public is severely impacted by air pollution even at the current exposures. Our understanding of the magnitude of the problem is hampered by a lack of analysis, uncertainties in the science, and the temporal characteristics of the research. However, it is apparent there are serious, life-impacting health effects at current exposure levels. We have had limited success controlling ongoing exposures during the past 50 years and today the controls are nowhere near where they must be to address the wide range of health endpoints that are being defined even as we write these documents. To assume that a range of controls including regulations, new technologies, voluntary approaches, and other incentives are going to correct a problem that has never been fully corrected to date and which we estimate is worse than previously understood is not a satisfactory policy analysis. We do not have a clear and documented understanding

of the magnitude and scope of the problem, so it is impossible to assume that the controls will adequately impact the health consequences.

In terms of the potential effectiveness of controls, there are many unresolved issues: for example, the elimination of old diesels will not necessarily proceed at a rapid pace, even with an influx of public monies; there may be questions related to diesel trucks from Mexico; having the cleanest marine vessels being directed to California service is a goal not a reality even in the foreseeable future; and maximum use of shore power or alternative controls represents a goal to be achieved. In fact Table III-13 (with a typo, 2105 versus 2015) represent reasonable goals, but to state there will be "highly effective controls on main and existing engines" to be begun around 2015-2020 is optimistic.

CARB Staff Response: These issues are addressed in the Goods Movement Emission Reduction Plan (the main report).

5. **There is inadequate attention to "vulnerable populations," impacted communities and occupational exposures** even though there is mention of them. It is not sufficient to acknowledge problem areas and then go on as though this constitutes a meaningful addressing of the issues.

   CARB Staff Response: Our health assessment specifically addresses the populations most vulnerable to the impacts of air pollution (i.e., elderly, those with pre-existing disease, children, etc.) and the communities most impacted by the ports and goods movement-related sources. The major health endpoints (i.e., premature deaths, hospitalizations) and others are quantified. Health endpoints without a strong "weight of evidence" for a causal association with air pollution are covered in sensitivity analyses or qualitative discussions. As California's air quality management agency, our analysis focuses on outdoor air pollution. Other agencies have expertise and responsibilities with occupational exposures.

6. **Cost-benefit:** One of the aspects of the document that I found particularly frustrating was the absence of a clear documentation of the measures contemplated to stimulate or discourage additional goods-movement activity and the expected goods-movement that would be expected to happen with and without those stimulatory/discouraging measures. Stimulatory measures include permitting various expansions of port facilities and state actions to build the supporting infrastructure of roads needed to enable the additional goods-movement activities to take place. The authors seem to have assumed one particular scenario for the growth of goods movement activity to about 2020 and made some baseline assessment of the direct impacts of the changing emissions with and without implementation of some not-fully-defined set of abatement measures.

   Appendix A states:

   "According to Phase I and other preliminary environmental assessments, it was estimated that without new pollution prevention interventions, a tripling in trade at the Ports of Los Angeles and Long Beach between the years 2005 and 2020 would result in a 50% increase in nitrogen oxide (NO_X) emissions and a 60% increase in diesel

particulate matter (PM) from trade-related activities, during a time when overall air pollution will decrease (CARB 2005a)."

The reason why it is important to evaluate the proposed stimulatory/enabling actions that are part of the original plan for increased goods-movement activities is that there are numerous economic and emissions/health effects side effects that would be different for different levels of stimulation/facilitation of increased goods movement. For example, increases in goods movement through the ports of Los Angeles and Long Beach will clearly add appreciable truck traffic to the already-congested Long Beach freeway and other nearby roads. This means either (1) increased traffic delays and local emissions as cars and non-port trucks necessarily proceed more slowly in the areas of increased traffic, and/or (2) increased state and local costs to expand road capacity in the affected area. While the latter is anticipated, the impact is still unclear.

CARB Staff Response: In the Proposed Emission Reduction Plan, growth factors are discussed along with other issues.

In addition, there is no attempt to quantify the additional exposures to residents or even commuters traveling on freeways or roads in gasoline vehicles with increased goods movement (diesel truck) activity, as acknowledged in one passage at the end of section C in Appendix A:

"Quantifying the increased in-vehicle exposures due to increased goods movement traffic emissions is beyond the scope of this report, but needs to be taken into account before total exposure impacts can be considered fully quantified."

This is unacceptably vague since it does not lay out a process for how the wide range of uncertainties is going to be addressed while the process appears to be moving forward rapidly.

CARB Staff Response: The impact of near-source and in-vehicle exposures are implicit in the concentration-response relationships which correlate observed health effects (due to all air pollution sources) with personal exposures as represented by ambient outdoor monitors. Since our analysis related pollutant emissions to health effects (e.g., tons of diesel PM per premature death), for situations where emissions and personal exposures go up (due to goods movement emissions growth) or down (in response to emission control measures), our health assessment would represent the health impacts of changes in near-source and in-vehicle exposures.

The Executive Summary commendably quantifies and presents uncertainty ranges and draws on credible studies of the chronic mortality implications of particulate exposures. However, it does not seem to compare the expected health and economic impacts with and without whatever expansion is contemplated in the California international goods movement and it does not address health consequences fully.

I suggest that the ARB and other relevant agencies (OEHHA, DHS, DIR) do additional analyses of what California air quality would be like if there was not a tripling of trade in

the next 20 years and if emission and other controls were put in place. Only with such comparative data can scientists, public health officials, environmental policymakers, and legislators fully understand the impacts of the decisions the State is making to stimulate and accommodate increasing international trade.

CARB Staff Response: Our analysis is consistent with approaches used for State Implementation Plans where we attempt to account for population, vehicle, and industrial growth and the impact of adopted and proposed emission controls to meet clean air targets (e.g., ambient air quality standards).

Other related issues:

a. Unfortunately, the document does not address a quantitative estimation of the contribution of diesel particulates to carcinogenesis and infant mortality. The likelihood is that these effects are appreciable. As an additional example, there is no estimate of the impact of vapors, e.g., naphthalene, which has been identified as a carcinogen and for which OEHHA has developed risk values. The issues of interactive effects are touched upon but, again, they are acknowledged but not addressed, thereby making the section more like a brief review article than an in-depth analysis. In general, there is an over-reliance on a limited set of studies.

CARB Staff Response: Staff has added infant mortality in the sensitivity discussion of the quantitative assessment.

b. In regard to the studies used, I think there is no reason whatsoever to not use the more current Jerrett study instead of the Pope study. Dr. Jerrett fully discusses this issue in his comments. The Emission Reduction Plan document states: "Further studies to confirm the results of this study are warranted". This seems to me to be a classic state of avoidance. Of course the Jerrett study should be used; it represents one of the seminal contributions to this field and it specifically considers measurements of PM2.5 in California.

CARB Staff Response: Staff addresses Jerrett's work in the discussion on sensitivity.

c. I disagree with sections of the Appendix that represent essentially literature reviews of health endpoints, e.g., cardiovascular disease, lung cancer etc. In addition the section on health and environmental justice has no apparent context. There should be a discussion about each topic (endpoint) in the context of what we know, whether it will be impacted by goods movement expansion, and if strict quantitative estimates cannot be made there should be some bounding estimates made.

CARB Staff Response: Staff has revised the discussion of these morbidity endpoints.

d. The approach to risk quantification is limited and while the literature review acknowledges in part the research that has emerged in the past 10-15 years it does not seek to use the information creatively to generate a more complete picture of the health consequences of PM particularly, and there is no attempt to discuss the role of the vapor phase toxicants except in the context of ozone. There needs to be a greater attempt to make estimates of risk based on the more recent studies even

with indirect endpoints. Attention needs to be addressed to population distributions where the greatest impact will be on the individuals at the tails of the distribution.

CARB Staff Response: After a thorough literature review, new health endpoints that met specific criterion for inclusion in the quantitative analysis (using "weight of evidence" similar to U.S. EPA's methodology) were included. Premature death was the endpoint of greatest interest, and the literature tells us that PM (including DPM and ozone appear responsible for the vast majority of air pollution-related deaths.

e. Children's Health Study (CHS) results and lung function/lung function growth issues: The health endpoints in the CHS are not defined in terms of readily quantifiable parameters such as mortality, asthma attacks, etc., but they represent endpoints with health consequences throughout a child's lifetime. A child with decreased lung function may have no clinical manifestations that are measurable, but those at the ends of the distribution may be severely impacted as a result of their greater susceptibility. Therefore reporting the impact of PM2.5 or elemental carbon on lung function is a meaningful endpoint especially when one considers the health effects that may occur over an individual's lifespan as lung function further declines.

CARB Staff Response: Staff has carefully reviewed the literature from CHS. Some asthma-related endpoints are considered in a sensitivity analysis to avoid double-counting (with lower respiratory symptoms and school loss days, for example).

f. Cardiovascular disease: First, almost the entire section is written using a secondary reference (Brook, 2004). That is not appropriate. Second, there is no attempt to conduct a complete review of the evidence that relates cardiovascular disease and air pollution. It is apparent that this area is extremely important at this stage and impacts a very large number of people. This is an endpoint of major consequence, and it is not addressed fully by looking at mortality. The impact of extended disability and diminished quality of life over time is particularly meaningful and it is not discussed anywhere in the document. In my view there is significant morbidity associated with PM related cardiovascular effects occurring under current air pollution conditions and it is likely to become considerably worse with goods movement expansion. There are a range of endpoints that can be estimated on a quantitative or semi-quantitative basis, e.g., fibrinogen, inflammatory measures, lipid oxidation, etc. While these do not constitute specific health endpoints they can be estimated and the implications discussed.

CARB Staff Response: Staff has expanded the list of PM-related health endpoints to consider cardiovascular disease.

g. The discussion of "Community Health Impacts" again reads like a literature review. There is no attempt whatsoever to develop any quantitative inferences as a result of the cited studies. The section acknowledges that goods movement may be a factor in certain health endpoints, but it begs the question overall.

CARB Staff Response: Our health assessment specifically addresses the populations most vulnerable to the impacts of air pollution (i.e., elderly, those with pre-existing disease, children, etc.) and the communities most impacted by the ports and goods

movement-related sources. The major health endpoints (i.e., premature death, hospitalizations) and others are quantified. Health endpoints without a strong "weight of evidence" for a causal association with air pollution are covered in sensitivity analyses or in qualitative discussions.

h. "Cumulative impacts are very likely to be experience (sic) by communities living in close proximity to goods movement-related activity." That is an important finding that requires in-depth discussion, but the rest of the paragraph and the one that follows are not developed to address the topic. The rest of that section addresses very briefly multiple exposures which is not the central issue to be considered when the issue of community impacts is the key topic.

CARB Staff Response: Our health assessment includes the impact of multiple sources, pollutants, and health endpoints. There is an implicit assumption of linearit; for example, premature deaths due to diesel PM, other PM2.5 sources, ozone, etc. are added together. But we are not aware of any research to suggest otherwise.

i. Unless I missed it, there is no discussion of the literature and the implications for neurological disease based on the data that has emerged from our SCPC laboratories, Rochester, Harvard and other PM centers. While much of this work is preliminary it should be acknowledged because there is potential for severe consequences.

CARB Staff Response: Staff considered this endpoint in our selection of health endpoints and studies for quantification. However, based on the selection criteria, it was decided that the weight of evidence was deemed insufficient for quantification at this time.

j. Development effects: why is there no attempt to quantify risk? This is an extremely important area and it does not get the attention that it deserves. Again, it is treated like a literature review rather than in the context of a document that seeks to assess risk associated with air pollution exposure especially that associated with goods movement.

CARB Staff Response: Staff considered this endpoint in our selection of health endpoints and studies for quantification. However, based on the selection criteria, it was decided that the weight of evidence was deemed insufficient for quantification at this time.

k. Dr. Jean-Paul Rodrigue of Hofstra University has written recently about "Transportation Pollutants and Environmental Externalities" and he suggests that the following are relevant parameters for evaluation: loss of useful life, replacement and restoration costs, men-hours-wage losses, output/surface decrease, biomass restoration time losses, medical services costs and loss of life expectancy. One could identify additional parameters including morbidity of extended periods, lack of capacity, and other factors. If we are to make meaningful decisions about the impacts of goods movement on health and the economy all of these health consequences and surrogates of health endpoints need to be addressed.

CARB Staff Response: Staff has addressed these concerns by considering premature death, work loss days, and other health endpoints.

l. Research conducted over the past 10 years has clearly demonstrated that the health problems associated with air pollution are greater in scope, magnitude, and impact over that which has been understood by more limited, traditional approaches to air issues. The problems that have been identified are occurring at current levels of exposure; they are not reflections of the past. In addition there is new research which casts doubt on our previous approaches including the adequacy of mass based standards and that raise new issues, including the role of ultrafines especially as larger PM is reduced, and a wide range of new endpoints. Since we are just beginning to appreciate the magnitude of the current problem it is extremely problematic to make adequate estimates of what the consequences may be with a tripling of goods movement.

The State should consider development of a document that seeks to implement the recommendations prepared by the NRC in Estimating the Public Health Benefits of Proposed Air Pollution Regulations and even expand beyond the topics in that useful document to better understand the scope of the required analysis.

CARB Staff Response: Staff coordinates with U.S. EPA and other national experts on quantifying the health impacts of air pollution to assure that our methodology is inline with other works.

m. The document (IV-1) states "Table IV-2 shows an overall 44% reduction in statewide diesel PM emissions from ports and international goods movement with plan strategies between 2001 and 2020 despite growth. Although this is significantly below the stated goal (85%), staff estimates a much greater reduction in proximate exposures and health impacts during the same time frame."

The fact remains that the projected reductions will not reach the stated goal. The staff estimates about "greater reductions" may be optimistic since staff has not even quantified most of the health endpoints. Even assuming the best case scenario with "deaths avoided," there will still be a significant impact of goods movement on health including as estimated 420 premature deaths, 150 hospital admissions, 8100 asthma attacks, 74,000 work loss days, 53,000 minor restricted activity days and 170,000 school absence days, and in my view this represents a vast understatement of the consequences. In fact, we have a major health problem that currently exists irregardless of goods movement expansion and that problem will only become better documented over time.

I have not attempted to address the specifics of Appendix A, since I do not believe that document addresses the breadth of the issues. It may serve as a useful if incomplete exercise, but a more expansive approach is required with involvement of other Agencies and Offices. The problem is what is missing rather than what is presented. I am available for further discussion as needed. Thank you for asking for my input.

CARB Staff Response: The proposed emission reduction plan shows greater reductions (diesel PM down 79% compared to 44% in the draft plan, while NOx decreases 63% between 2001 and 2020 compared to 55% previously). Staff has also expanded the list of health endpoints quantified. Staff recognizes that the projected health impacts in year 2020 (based on existing control program without new plan strategies) may overestimate the impacts if the State ambient air quality standards for PM and ozone are attained by the year 2020.

Appendix 1.

I am attaching the comments from Michael Jerrett since I wish them to be considered with my comments as well. I think they reflect a high degree of sophistication in addressing the issues and they reflect my views in their entirety.

Original Message-----
From: Michael Jerrett [mailto:jerrett@usc.edu]
Sent: Saturday, December 10, 2005 5:42 PM
To: 'Bart Croes'; 'John R. Froines'
Cc: 'avol@usc.edu'; 'Arthur Winer'; 'Nino Kuenzli'; 'dhattis@aol.com'; 'Richard Bode'; 'Linda Tombras Smith'
Subject: RE: Goods Movement document - initial review with more to follow

Hi John and Bart,

Many thanks to both of you (John for your extension suggestion and Bart for your understanding). This extension will result in a more thorough and thoughtful review, and in the end a better study and methodology.

I will continue with my review and will try to get any suggestions about models that need to be rerun to you quickly. In my initial review, it seems that you did not use our recent ACS study form LA. Given that 70% of the deaths come from the South Coast Basin, I recommend that you conduct and report this estimates from the LA study as another credible (and probably more relevant) risk estimate for the California population. There could be two specific analyses:

1. One applying the estimate only to the South Coast and then blending in the higher total from that region with the rest of the state estimated from Pope et al. 2002; and
2. Another applying the LA estimates to the entire state.

Just to clarify what seems to be a misconception in the appendix document, the main estimates presented in the LA paper use EXACTLY the same model as Pope et al.

2002. These estimates are fit with a standard Cox regression model that controls for 44 individual covariates and stratifies for age, sex, race in the baseline. Thus if you want to use the estimates that are the same as the Pope study, then these are available. We intentionally used the same model so such comparisons (and risk estimates) would be available to policymakers for burden assessments and others interested in understanding why the risks in LA were higher.

All of the ecologic confounders and spatial models drive down the estimates or widen the confidence intervals, but they are still about twice as large as the estimates presented in Pope. If you choose to run the sensitivity models using the LA estimates suggested above, I would first use the same ones as Pope without the spatial adjustments. You could if you wanted also report the lower bound with maximal control for neighborhood confounders, but to do this correctly, you would need to account for the spatial variation in the ecologic confounders for the current population in California (which could be quite a chore). But you could report the lower estimate without the more complex analysis as another sensitivity test to supply a lower bound.

The argument currently in the document for not including the LA estimates could be criticized as logically inconsistent. If you did not use the LA estimate because it does not apply to the entire state, then why would another estimate from Pope et al. which includes 116 cities (many of which are very different in pollution mixture and population characteristics than CA)? In fact, if you were trying to match the analysis on the factors that can bias the risk estimates, then the LA study is arguably more even more relevant as the main estimate by almost all the criteria that matter: (a) the pollution mixture in LA is closer to the pollution mixture across all of CA than the mixture in the 116 cities in Pope et al. which is dominated by sulfate contrasts in the in the lower great lakes; (b) the underlying population characteristics are much closer in the LA study than again in the 116 cities; (c) the relative weight in the model given to CA in the Pope study is less than 10% of the total ACS population in the ACS study (that's my recollection, I'll get you exact numbers soon), while the LA study is 100% based on CA populations; and (d) the spatial resolution of your exposure assignment is if I understand it correctly more of an within-city assessment than a between-city contrast, so again the LA study is a closer match to the health risk assessment. On this last point, I have not reviewed the document in detail, but am relying on your earlier protocol and Arthur Winer's nice description in one of our meetings to discuss the protocol. For all of these reasons, conducting sensitivity analyses on the likely mortality reductions from the LA study estimates is important to the credibility and logical consistency of your chosen dose-response functions and the entire analysis.

Other Comments:

1. There is a potential problem with the narrow definition of port and goods movement activities. These activities have ramifications that go beyond the immediate

trains, trucks, and ships, which are the focus of your study. There are many automobile trips from workers traveling to and from their jobs which need to be taken into account. A more thorough and complete way to understand these impacts would be through an econometric computable general equilibrium model or at least an input-output model. This would give you some idea of the secondary and tertiary ramifications of goods movement. I'm certain that the Finance Department (or equivalent) would have calibrated such a model already, and if they have not, Dr. Sergio Rey of San Diego State University has one that I've used in similar research with him some time ago. I have co-authored a number of papers using the I-O and CGE approach and for the longer term methods development, it would be a good idea to expand this definition.

2. What about the impacts of airports? These are increasingly seen as a major source of pollution. These do not seem to be in the goods movement definition and they should be as far as I can tell.

3. There are a number of estimates that implicate NO2 as a potential source of health effects. Whether NO2 is the putative agent, interacts with other pollutants, or serves as good indicator of mobile source pollution is an open question, but I feel that the estimates of NO2 mortality could be added as a sensitivity analysis (although this raises the issue of overlap with the PM effects). The study by Nafstad et al. (2004) supplies mortality estimates for a Norway, and it would be worth investigating what inclusion of NO2 does to your estimates. Or you could use recent studies by Burnett et al. for time series estimates (again a sensitivity analysis)

4. The comment that there "strong" associations between air pollution and health may be an overstatement. Strength of association in epidemiology relates to dose-response coefficient size. When the size is only a 1% increase for time series mortality estimates over a 10 ug/m3 contrast, it is difficult to call this "strong". Even the 6% increase in Pope et al. is not that large an effect (say compared to smoking or ETS for example). The estimates are more properly called "consistent" between places and biologically plausible in the Hill terminology of causation. The key point is that even when the relative risks are small, they affect large populations and as a result have the potential to have sizable impacts on mortality and morbidity. Rose has a famous paper that discusses this point.

5. There are a number of other papers that should be cited supporting the health effects of living near roads: Hoek et al. 2002 (Lancet); Finkelstein et al. 2004 (AJE); Nafstad et al. 2004(EHP)). All of these deal with mortality and therefore are very relevant to your assessment.

6. Table A4 should include ischemic heart disease as a separate category for premature death. It is associated with air pollution more strongly than CPD, and in general, respiratory deaths are not usually elevated (6 cities study, my studies with Finkelstein in Hamilton and the ACS study all show this).

7. For ozone, there is a more tenuous relationship, at least to mortality. The ACS studies do not find a significant association. I will read more on this, but my initial reaction is that you could again be seen as inconsistent. If you are going to use time series estimates for ozone mortality (which are smaller) and then chronic estimates for PM (bigger), someone could ask, why have you not used time series for mortality, which would dramatically reduce your estimates. But if you use chronic estimates for ozone, they are not significant. You need to be consistent or it will look like you are just grabbing whatever seems largest (and I know from all the hard work and thoughtful discussion in the document that is not the intent). I can say that our new ACS analysis, which is under preparation, does indicate an ozone effect on all-cause mortality for the national level study, but that is not going to be out for some time.

CARB Staff Response: Staff addresses all of Dr. Jerrett's comments in another section of this chapter.

2. Professor Jane V. Hall, California State University, Fullerton

1. Because mortality tends to drive the aggregate results of benefit assessments, it is especially important that valuation of this endpoint is well supported. When I take the most recent EPA value for the value of a statistical life (VSL) of $5.5 million in 1999 dollars (from the EPA March 2005 RIA, 4-51), and adjust it for the CPI and California real per capita income changes from 1999 through 2004 (from DOF), further adjusting PCY with an income elasticity of 0.4, I end up with $6.4 m for VSL.

CPI 1999 = 166.6

CPI 2004 = 188.9

% PCY 2000 = 6.2..... Adj. 2.5

% PCY 2001 = (0.8) ..... Adj. (0.3)

% PCY 2002 = (1.6) ..... Adj. (0.6)

% PCY 2003 = (0.1) ..... Adj. (nil)

% PCY 2004 = 2.7..... Adj. 1.1

This diverges considerably from the draft report's value of $8.2m in 2005, in part because of the assumption in the report that 0.8% is the appropriate annual rate of real income growth to extrapolate values forward. Looking at the past five years, this is closer to 0.5%, when also adjusted for income elasticity.

CARB Staff Response: We agree that the mortality endpoint must be well-supported. ARB uses the VSL estimate recommended by U.S. EPA's "Guidelines for Preparing

Economic Analysis," [1] *($4.8 million 1990 dollars), as the starting point for calculation of mortality benefits. This value is based on Viscusi's 1992 meta-analysis of 26 WTP studies, and was used in all U.S. EPA regulatory impact analyses through 2003.*

As you point out, more recent air regulatory impact analyses from U.S. EPA [2] *have applied another, lower, VSL estimate, ($5.5 million 1999 dollars), based on more recent meta-analytical literature.*

However, U.S. EPA staff say that the new VSL estimate, which emerged from negotiations between U.S. EPA and OMB, has not yet been assessed by the Environmental Economics Advisory Committee of U.S. EPA's Science Advisory Board (SAB). Until SAB has reviewed and endorsed the new estimate, we prefer to continue using the last VSL estimate approved for use by the SAB.

Since the draft GM report was issued, we have adopted a different set of CPI indices, (annual, national, all-item, urban CPI from U.S. BLS), to adjust for inflation through 2005. (The draft report used month-of-February values.) In addition, (as discussed below), we have adopted U.S. EPA's most recent adjustment factor for real income growth, which incorporates both a lower implicit annual growth rate (0.6% rather than 0.8%) and a 0.4 elasticity factor.

When we compare income and elasticity-adjusted VSL values based on both the established VSL estimate and the more recent VSL estimates, the gap between ARB's proposed VSL and that in your comment is narrowed. For example, you have estimated $6.4 million at the 2004 income level, expressed in 2004 dollars. Our revised value for the 2004 income level expressed in 2004 dollars would be $7.5 million.

The gap between your estimate and ours is narrowed by our revisions, but it persists because ARB has not adopted the U.S. EPA's more recent, ($5.5 million 1999 dollars), VSL estimate. At the 1990 income level, the gap amounts to $0.6 million 1999 dollars.

2. I cannot determine whether future values were adjusted for income elasticity as well as real income growth. Given that there was apparently no adjustment, either this should be changed or a sound explanation should be given for adopting EPA's approach on income adjustment except for the elasticity adjustment.

   CARB Staff Response: The future values (for years 2005-2020) provided in the draft Goods Movement report take into account both real per capita income changes as well as income elasticity. In our draft report, however, we adopted the 1990-2020

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[1] Page 90, "Guidelines for Preparing Economic Analysis" U.S. EPA, September 2000 http://yosemite.epa.gov/ee/epa/eed.nsf/webpages/Guidelines.html

[2] Sometime between publication the "Draft Regulatory Analysis: Control of Emissions from Nonroad Diesel Engines" (April 2003) and the "Final Regulatory Analysis: Control of Emissions from Nonroad Diesel Engines" (May 2004), the new VSL value was adopted. See Chapter 9, page 9-160, footnote cc. http://www.epa.gov/nonroad-diesel/2004fr/420r04007j.pdf

adjustment factor, (1.262), from EPA's draft nonroad diesel analysis. The final version of that EPA analysis revises the adjustment factor downwards, (to 1.201), and we use this revised factor in the attached spreadsheet. It is our understanding that the U.S. EPA adjustment factors used to account for projected real income growth[3] incorporate a central elasticity estimate of 0.40.

3. For school absences, EPA has combined several studies to estimate that the average duration is 1.6 days (EPA 2005 4-38) and is using this assumption in estimating days of absence. I cannot tell if the draft ARB report assumes each absence is one day, or something else, but the EPA approach is well supported.

   CARB Staff Response: For school absences, the draft ARB report uses an average duration of 1.6 days as you suggest.

4. The report acknowledges that some quantifiable effects are not quantified. Given the established basis for estimating and valuing several significant endpoints, I am not clear on why they were omitted, other than the possibility of overlap (double counting). Onset of chronic bronchitis, in particular, should be included. Respiratory symptoms should also be included, and could be adjusted for possible overlap by netting out respiratory-related hospitalizations. Acute bronchitis could be included and similarly adjusted.

   CARB Staff Response: Indeed, some endpoints were not included to avoid double-counting. In other cases, they were not included because the overall weight of evidence was deemed insufficient at this time for quantification. Nonetheless, staff has expanded the list of quantified health endpoints to include hospitalizations (due to respiratory and cardiovascular causes), asthma and other lower respiratory symptoms, and acute bronchitis. Staff also explained other potential health endpoints in a sensitivity discussion.

3. Aaron Hallberg, Abt Associates, Inc

1. It seems to me that one of the major sources of uncertainty with the assessment is the assumption that there is a direct correspondence between emissions and exposure. That is, actual estimates of exposure are only generated for the year 2000, and are used just to generate impacts per ton of emissions. All of the health impact numbers presented are then based on the assumption that a linear relationship holds between emissions and health impacts (with a correction for population growth), which is certainly not the case. Given that you did not have the time or resources to use air quality modeling, this may well be the best approach available to you, but you should still discuss the uncertainties involved.

   CARB Staff Response: We will discuss this uncertainty.

2. It appears that Ozone impacts were calculated just for ozone levels above the standard, while PM impacts were calculated all the way down to zero. Is this correct? If so, it seems strange to me and should probably receive some discussion. At the very least, it

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[3] See footnote A of Table 9A-16 on page 9-121 of "Final Regulatory Analysis: Control of Emissions from Nonroad Diesel Engines" (May 2004), as well as Table 9A-15 on p. 9-119.

should be mentioned that the disproportionate impact of PM in the appendix is partially (largely?) an artifact of this distinction between the two pollutants. That is, if ozone impacts were calculated down to background they would be much higher.

CARB Staff Response: Staff agrees and has modified the ozone methodology to associate only the portion of the NO_X and ROG inventories needed to reduce to attain the ozone standard with the health benefits of attaining the standard. This revised methodology is now consistent with PM.

3. There are two major issues regarding demographic projections:

    a. The composition of California's population is going to change quite a bit between 2005 and 2030, but it appears that only a simple total population ratio was used to project impacts in future years. In general, I would expect this to lead to an underestimate of various health impacts, and especially of premature mortality.

    b. Baseline incidence rates will also change between 2005 and 2030, but it appears that they are assumed to be constant. In particular, baseline mortality rates should decline, perhaps substantially, leading to an overestimate of premature mortality.

    c. Note that these two issues may more or less cancel each other out, but it is unclear what the overall impact might be. You may want to try to account for these changes, or at the very least you may want to discuss them as a potential source of uncertainty.

    CARB Staff Response: We agree with the above suggestions. It's unclear what other approaches to addressing population shifts are do-able. Without any documented or peer-reviewed future demographics information, staff cannot account for these changes aside from population growth.

4. Many health endpoints typically included in U.S. EPA analyses were left out of the assessment, including Infant Mortality, Chronic Bronchitis, Acute Myocardial Infarction, Cardiovascular Hospital Admissions, Acute Bronchitis, Lower and Upper Respiratory Symptoms. The exclusion of chronic bronchitis is especially troublesome, as it is typically the second highest component of the economic valuation (after premature mortality). Additionally, the chronic bronchitis study typically used by U.S. EPA (Abbey et al, 1995) was actually done in California. See, for example, the Clean Air Interstate Rule regulatory impact analysis at http://www.epa.gov/interstateairquality/pdfs/finaltech08.pdf.

    CARB Staff Response: Staff has re-evaluated the literature and selected additional health endpoints in our analysis. They include: hospitalizations due to respiratory and cardiovascular causes, asthma and other lower respiratory symptoms and acute bronchitis due to PM.

5. The low and high estimates of economic impacts are presented as 5th and 95th percentiles of "…an integrated analysis of uncertainties in human health concentration-response functions and the economic values…" (pg. A-25). The method of generating these values, however, seems incorrect to me. If I am reading it correctly (pg. A-48), you are simply multiplying the 5th percentile estimate of cases by the 5th percentile of the unit value distribution to generate the 5th percentile of the combined distribution. The

typical approach for this process, assuming that the unit value distribution and the distribution of the C-R function coefficient are independent (and I see no reason not to make this assumption), would be to run a Monte Carlo simulation to generate the composite distribution and then pull the 5th percentile, the mean, and the 95th percentile from this composite. If the two distributions are not skewed, the mean value from this procedure should be very close to the product of the two means. The 5th and 9th percentiles, however, will typically be quite different from the simple product of the 5th and 95th percentiles.

CARB Staff Response: Staff has carefully considered the distributions of the C-R function as well as the unit valuations. In the proposed plan, staff has developed a procedure for propagating the uncertainties from both sources into the economic valuations of premature deaths. Staff also provided a detailed explanation of this step in Appendix A.

6. You might consider updating your premature mortality unit value – EPA has more recently used (see the Clean Air Interstate Rule regulatory impact analysis referenced above) a normal distribution with mean of $5.5 million (1999 dollars) and 95% confidence interval of $1.0 million to $10 million. Obviously, the choice of a premature mortality unit value will largely drive your overall economic impacts, so it is quite important.

CARB Staff Response: ARB uses the VSL estimate recommended by U.S. EPA's "Guidelines for Preparing Economic Analysis," [4] *($4.8 million 1990 dollars), as the starting point for calculation of mortality benefits. This value is based on Viscusi's 1992 meta-analysis of 26 WTP studies.*

As you point out, more recent regulatory impact analyses from U.S. EPA[5] *have applied another VSL estimate, ($5.5 million 1999 dollars), based on more recent meta-analytical literature. However, neither OMB nor EPA's Office of Transportation and Air Quality have explained the reasons for this change.*

U.S. EPA staff say the new VSL estimate emerged from recent negotiations between U.S. EPA and OMB, and has not yet been assessed by the Environmental Economics Advisory Committee of U.S. EPA's Science Advisory Board (SAB). Thus far, EPA has

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only used the new VSL number in economic analyses of air regulations. Until SAB has reviewed and endorsed the new estimate, we prefer to continue using the last VSL estimate approved for use by the SAB.

In a February 7, 2006 Email to ARB staff, Jim DeMocker of U.S. EPA's Office of Policy and Review states, "I will also say that, while I would agree the $5.5 million central value is "in the neighborhood", I do not believe it is an appropriate value to use in regulatory analysis. Doing so represents a deviation from prevailing Agency Economic Guidelines and ignores the most recent advice from one of the outside expert panels which advise us on this topic."

7. I found the presentation of future economic benefits on page A-6 quite confusing at first – you present a single central estimate of premature mortality, but two central estimates (or a central estimate range) for economic benefits. Later I realized that these two estimates were generated using different discount rates, but this is not explained until page A-56. Indeed, table A-12 is similarly confusing. I would recommend explicitly presenting these two sets of numbers as different central estimates, rather than as a range. I would also present the confidence intervals separately – it is still not clear to me what these represent.
    a. Does the 5th percentile come from the (lower) 7% discount rate and the 95th from the (higher) 3%? In this case, in what sense do these numbers represent a 95% confidence interval?
    b. Also – I think you are really presenting a 90% confidence interval (5th – 95th), not a 95% confidence interval (2.5th – 97.5th). This should probably get updated throughout.

CARB Staff Response: Staff has modified the presentation and the explanation of the economic values so that the reader can follow the derivations better in the final plan.

8. On page A-82, you say:

"The current methodology used a power of 2.5 in order to optimize the interpolations… Further, the current methodology uses a minimum of 10 monitoring stations and up to a total of 15 in weighting the results to estimate the concentration at each census tract."

There are two issues here, which it probably makes sense to discuss in the appendix. The first is in what sense using a power of 2.5 (in the inverse distance weighting) "optimizes" the interpolations. I can see how it gives greater weight to nearby monitors, but was some test run which determined that this was the "best" value in some sense, or is it simply based on intuitions about nearby monitors giving more accurate results? Secondly, using the old algorithm for selecting monitors to use in the interpolation process was quite straightforward – everything within 50 km was used. The new procedure is not similarly straightforward – how are the 10 to 15 monitors selected? Are

they simply the closest monitors, within some radius? Or are they the monitors that "surround" the interpolation site in some sense? When are 10 used and when 15?

CARB Staff Response: In the final plan, staff has modified the interpolation scheme to use the power of 2 (i.e. inverse distance squared) and when no monitor is found within the 50-km radius, the nearest 3 monitors are used.

9. On page A-90 you mention that annual concentration statistics are reported as geometric means. Are these geometric means used in combination with C-R functions to generate health impacts, or are they strictly for reporting? I would not recommend using them in C-R functions unless the coefficients in these functions were developed using geometric means.

   CARB Staff Response: Particle size distribution tends to be a log-normal distribution. The log-normal distribution often yields good approximations of size distributions with clearly distinct modes under ideal conditions. Also, the annual California ambient air quality standard for PM is based on the geometric mean (useful for characterizing lognormal data). Thus, the geometric means of PM10 sulfate mass concentrations were calculated for each site for the period of 1998. Comparison of the annual geometric means of PM10 sulfate and nitrate mass with that of arithmetic mean values reveal no significant differences.

10. In response to my previous comment on interpolation to the census tract, you mentioned that "…population-weighted exposures were developed at the county and basin level, consistent with the higher levels of spatial aggregation used in the epidemiologic studies." The issue, however, is that the population-weighted exposure estimates are based on census-tract population levels and census-tract pollutant concentrations (at least on my reading of the appendix – if this is not the case, you might want to re-write this section). Again, this assumes that people are exposed to ambient pollution according to where they live, and is not consistent with the spatial scales typically used in epidemiologic studies. This is probably not a huge issue, but it seems like you are doing a whole lot of work to generate numbers which are not likely to be any more accurate than a simple county- or basin-wide average.

    CARB Staff Response: We maintain that while the intermediate steps in performing the interpolations requires census-tract concentrations, the end products are county-wide and basin-wide estimated concentrations that get applied to the CR functions.

4. Professor Michael Jerrett, University of Southern California

In my initial review, it seems that you did not use our recent ACS study from LA. Given that 70% of the deaths come from the South Coast Basin, I recommend that you conduct and report this estimates from the LA study as another credible (and probably more relevant) risk estimate for the California population. There could be two specific analyses:

One applying the estimate only to the South Coast and then blending in the higher total from that region with the rest of the state estimated from Pope et al. 2002; and Another applying the LA estimates to the entire state.

Just to clarify what seems to be a misconception in the appendix document, the main estimates presented in the LA paper use EXACTLY the same model as Pope et al. 2002. These estimates are fit with a standard Cox regression model that controls for 44 individual covariates and stratifies for age, sex, race in the baseline. Thus if you want to use the estimates that are the same as the Pope study, then these are available. We intentionally used the same model so such comparisons (and risk estimates) would be available to policymakers for burden assessments and others interested in understanding why the risks in LA were higher.

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The argument currently in the document for not including the LA estimates could be criticized as logically inconsistent. If you did not use the LA estimate because it does not apply to the entire state, then why would another estimate from Pope et al. which includes 116 cities (many of which are very different in pollution mixture and population characteristics than CA)? In fact, if you were trying to match the analysis on the factors that can bias the risk estimates, then the LA study is arguably more even more relevant as the main estimate by almost all the criteria that matter: (a) the pollution mixture in LA is closer to the pollution mixture across all of CA than the mixture in the 116 cities in Pope et al. which is dominated by sulfate contrasts in the in the lower great lakes; (b) the underlying population characteristics are much closer in the LA study than again in the 116 cities; (c) the relative weight in the model given to CA in the Pope study is less than 10% of the total ACS population in the ACS study (that's my recollection, I'll get you exact numbers soon), while the LA study is 100% based on CA populations; and (d) the spatial resolution of your exposure assignment is if I understand it correctly more of an within-city assessment than a between-city contrast, so again the LA study is a closer match to the health risk assessment. On this last point, I have not reviewed the document in detail, but am relying on your earlier protocol and Arthur Winer's nice description in one of our meetings to discuss the protocol. For all of these reasons, conducting sensitivity analyses on the likely mortality reductions from the LA study estimates is important to the credibility and logical consistency of your chosen dose-response functions and the entire analysis.

CARB Staff Response: Jerrett et al. (2005) found higher estimate for premature death associated with PM exposures than the national study by Pope et al., (2002), but greater uncertainty. Several additional studies have either just been published or will be in the next few months. ARB staff intends to review all of these studies and will solicit the advice of the study authors and other experts in the field and U.S. EPA to determine how to best incorporate these new results into our future assessments. We addressed the new study in a sensitivity discussion.

Other Comments:

There is a potential problem with the narrow definition of port and goods movement activities. These activities have ramifications that go beyond the immediate trains, trucks, and ships, which are the focus of your study. There are many automobile trips from workers traveling to and from their jobs which need to be taken into account. A more thorough and complete way to understand these impacts would be through an econometric computable general equilibrium model or at least an input-output model. This would give you some idea of the secondary and tertiary ramifications of goods movement. I'm certain that the Finance Department (or equivalent) would have calibrated such a model already, and if they have not, Dr. Sergio Rey of San Diego State University has one that I've used in similar research with him some time ago. I have co-authored a number of papers using the I-O and CGE approach and for the longer term methods development, it would be a good idea to expand this definition. What about the impacts of airports? These are increasingly seen as a major source of pollution. These do not seem to be in the goods movement definition and they should be as far as I can tell. There are a number of estimates that implicate NO2 as a potential source of health effects. Whether NO2 is the putative agent, interacts with other pollutants, or serves as good indicator of mobile source pollution is an open question, but I feel that the estimates of NO2 mortality could be added as a sensitivity analysis (although this raises the issue of overlap with the PM effects). The study by Nafstad et al. (2004) supplies mortality estimates for a Norway, and it would be worth investigating what inclusion of NO2 does to your estimates. Or you could use recent studies by Burnett et al. for time series estimates (again a sensitivity analysis). The comment that there "strong" associations between air pollution and health may be an overstatement. Strength of association in epidemiology relates to dose-response coefficient size. When the size is only a 1% increase for time series mortality estimates over a 10 ug/m3 contrast, it is difficult to call this "strong". Even the 6% increase in Pope et al. is not that large an effect (say compared to smoking or ETS for example). The estimates are more properly called "consistent" between places and biologically plausible in the Hill terminology of causation. The key point is that even when the relative risks are small, they affect large populations and as a result have the potential to have sizable impacts on mortality and morbidity. Rose has a famous paper that discusses this point.

CARB Staff Response: Staff appreciates the thought and carefully selected the studies (and the associated concentration-response functions) that report strong associations between air pollution and health. The strength of the relationship between NO_2 exposure and mortality is unclear at this point.

There are a number of other papers that should be cited supporting the health effects of living near roads: Hoek et al. 2002 (Lancet); Finkelstein et al. 2004 (AJE); Nafstad et al. 2004(EHP). All of these deal with mortality and therefore are very relevant to your assessment.

CARB Staff Response: By calculating health impacts due to air pollution exposures, we implicitly address the effects of living near roads as combustion-related sources near roadways contribute to the exposure levels.

Table A4 should include ischemic heart disease as a separate category for premature death. It is associated with air pollution more strongly than CPD, and in general, respiratory deaths are not usually elevated (6 cities study, my studies with Finkelstein in Hamilton and the ACS study all show this). For ozone, there is a more tenuous relationship, at least to mortality. The ACS studies do not find a significant association. I will read more on this, but my initial reaction is that you could again be seen as inconsistent. If you are going to use time series estimates for ozone mortality (which are smaller) and then chronic estimates for PM (bigger), someone could ask, why have you not used time series for mortality, which would dramatically reduce your estimates. But if you use chronic estimates for ozone, they are not significant. You need to be consistent or it will look like you are just grabbing whatever seems largest (and I know from all the hard work and thoughtful discussion in the document that is not the intent). I can say that our new ACS analysis, which is under preparation, does indicate an ozone effect on all-cause mortality for the national level study, but that is not going to be out for some time.

CARB Staff Response: The overall estimates of premature death from all causes (non-accidental) presented in our report cover the subcategory of ischemic heart disease.

5. Dr. Melanie Marty, Office of Environmental Health Hazard Assessment

General Comments: Overall, the report is clearly written and readable by the educated public. However, there is a tension between writing too much for the lay reader and not enough for technical reader. We understand that this document is meant more for the lay audience but perhaps a few additional details may be helpful. It is a good report with much information and provides a vision of potential future controls and their utility. However, more could be added about alternative fuels and other health effects related to gaseous constituents of diesel engine exhaust.

CARB Staff Response: In the final plan, staff has expanded the list of PM-related health effects to include hospitalizations due to respiratory and cardiovascular causes, asthma and other lower respiratory symptoms and acute bronchitis.

1. In the summary description of the health impacts they specify that they are interested in calculating only the impacts of internal transportation systems (trucks, rail) in so far as they reflect movement of goods in intercontinental trade. However, in the body of the report, it is several times pointed out that the same transportation systems that handle the imports and exports also handle movement of goods within California and the United States as a whole. So, any regulatory actions which improve the health impacts of goods movement related to intercontinental trade will also improve the health impacts of goods movement related to internal trade. Although it is clearly of interest to identify that fraction of internal goods movement which is related to the intercontinental trade via

the ports when considering projections of trade volumes, attribution of costs and so on, it seems illogical not to also present "up front" the entire benefit to be obtained from regulations or other measures which mitigate the health impacts of any goods movement regardless of the source of the cargo.

CARB Staff Response: In the proposed plan, staff has considered all goods movement, international and domestic.

2. Consideration of alternative strategies for mitigation of health impacts of internal transportation concentrates almost exclusively on regulations, market interventions and voluntary agreements aimed at reducing the impact of pollution from diesel-powered equipment. The considerable efforts by various interested parties, including CARB itself as well as SCAQMD and other air districts to develop alternative fuel vehicles are almost entirely ignored. While these efforts to mitigate diesel impacts are clearly appropriate and necessary, there should also be a place for application of zero-emissions and PZEV technologies such as electric power, and CNG- or even hydrogen-fueled vehicles. While these are likely to be longer-term options they have far greater potential to minimize health impacts, especially in local near-source situations where the health impacts are currently most severe. The analysis of port operations correctly identifies replacement of auxiliary diesel engine power by grid-derived electric power as a powerful tool to minimize health-damaging emissions from ships while in port, and on-port mechanical operations. It is implicit in this that grid-derived power is already at least partly derived from renewable and relatively non-polluting sources, and the attractiveness of this substitution is greatly increased if it is coupled with other State-wide efforts to increase the proportion of grid power from renewable and non-polluting sources which do not contribute to net CO2 emissions. This opportunity (or caveat, if grid power continues to rely on fossil fuels) should be made explicit in the report. The report fails to even mention electric traction as an option for mitigating rail impacts. This technology is ubiquitous in its application worldwide, and is even employed widely for passsenger rail systems in California, so the only barrier to its application is the cost of conversion, not feasiblity. In particular, its use for local switching equipment in rail yards and for tractor units on high-intensity metropolitan corridors (where its introduction would be easiest from a cost and regulatory point of view) has the potential to enormously reduce pollution impacts in precisely those areas where the impact of rail operations is currently most severe. As noted in the report, the overall scale of rail operations is presently not large except in some of these localized near-source areas, but is likely to become worse (including exceeding the per-ton-mile emission rate of projected "clean" trucks) unless cleaner rail operations are introduced. The calculation of truck and rail impacts in the report apparently fails to consider the substantial benefits of reducing traffic congestion in metropolitan areas (and thus secondarily reducing pollutants emitted from all mobile sources) if major "truck route" goods movement can be diverted to rail, although it is mentioned that several port authorities and air districts are examining this option. Use of electric traction for long-distance rail operations is a longer-term objective which obviously will require US involvement, but it does have the potential to significantly reduce pollution impacts statewide (and nationally), to reduce

CO2 emissions (especially if improved rail operations were to take travel market share from airlines and other fuel-intensive modes), and to reduce dependence of imported fossil fuels.

CARB Staff Response: The draft Emission Reduction Plan identifies a series of performance targets to reduce emissions from each sector and discusses a range of approaches that could be employed to reach those targets. Use of available zero and near-zero emission technologies is one of the approaches that would help meet our emission and risk reduction goals. The Plan discusses the potential applications for electric or hybrid-electric technology for ships at dock, harbor craft at dock, cargo handling equipment, and locomotive switcher engines at rail yards. Utilization of these technologies would have additional climate change benefits not quantified in the report. The Plan also acknowledges alternative fuels as another option that could be used to help meet the performance targets in any sector. ARB staff considered the specific suggestions in this letter, along with the many public comments on the strategies, in development of the proposed Plan.

3. Calculation of the health impacts of diesel emissions assumes that all such impacts are caused by the particulate component of the emissions (apart from the separate consideration of NO_X/ozone). While the diesel PM emissions are used as an accessible dose metric of total emissions, particularly when calculating cancer risk, they are by no means the only component of diesel exhaust with health impacts, especially when considering near-source exposures. Gaseous components of the exhaust (especially naphthalenes, butadiene and aldehydes) may contribute substantially to both cancer and non-cancer health impacts. They are also important contributors to ambient air toxics concentrations both of the emitted materials and their atmospheric transformation products. This latter issue does not appear to have been considered in the report, but is evidently an area of substantial impact and one about which at least some quantitative information is available (e.g., the data on ambient air concentrations of butadiene, formaldehyde, acrolein etc. in the South Coast air basin). The detailed consideration of NO_X emissions in the report appears to mainly address ambient levels and the interaction of NO_X and photochemically generated ozone, rather than also considering direct effect of such emissions near the sources. The report's disclaimer that health effects other than cancer and cardiovascular disease are not well-quantified as regards dose-response is not altogether unjustified, but much more could have been done by taking advantage of ARB's and OEHHA's extensive efforts to quantify health risks from ambient air toxics and Hot Spots emissions.

CARB Staff Response: Staff has used the best available scientific information to quantify cancer and non-cancer health impacts. The cancer risk addresses air toxics; the non-cancer health impacts were considered in conjunction with OEHHA.

4. Estimation of mortality impacts from cardiovascular disease in the report depends on a draft evaluation mainly by U.S. EPA of national data in an update to the Pope et al. study. The methodology and underlying data used are presented in a cursory and inadequate manner, and little consideration of the complexities of interpretation is presented. It seems inappropriate to rely on this non-peer reviewed estimate, (the peer reviewers quoted at the back of the document make it clear that they haven't been able to do much more than agree that the Pope et al. 2003 study is a reasonable basis for an estimate) in preference to the much more careful and extensive presentation in the recent CARB/OEHHA health effects analysis for the PM AAQS, which has been thoroughly peer reviewed and presents California-specific estimates. (This may be an important point: the report argues somewhere in the section that California PM is similar to other PM in the US, hence the national estimates are applicable, whereas in fact I understood that ambient PM in California was in fact considerably different from that found in other parts of the US, especially the East Coast cities.) The comments at the end of the report imply that something will be changed and the final will reflect use of the California AAQS analysis, but that isn't apparent in the current draft.

   CARB Staff Response: Staff has revised the sections on selecting health endpoints and studies, in consultation with U.S. EPA and OEHHA, in the final plan. Pope et al. 2002 estimate is the most widely cited study and used in health analysis to date. When it was published, CARB and OEHHA were in the process of finalizing the staff report on the PM AAQS; hence, it could not have been added through the formal peer review process. Nonetheless, as a follow-up to the ACS study (originally analyzed by Pope et al. 1995 and then re-analyzed by Krewski et al. 2000), it is quite appropriate to use in our analysis.

5. Some discussion appears in the report and peer review comments about "double counting" of mortality between overall mortality estimates based on PM and other cause-specific estimates (specifically cancer, since other mortality endpoints are not considered in any detail). It seems to me that something reasonable could have been done if the cause-specific analyses available in the AAQS report are used. (Bart O. could comment on this). The diesel-related mortality estimates are likely to be underestimates since they do not account for any effects besides particle-related mortality (based on the percentage of the total ambient PM assumed to be contributed by diesel) and cancer (quantified by diesel PM emissions). As noted previously, although some other effects are not as easily quantified (and some rely on a "safe level" determination rather than an absolute risk calculation), more could have been done. We do note the table relating other health effects that were not quantified and hope the quantified health effects could be expanded upon in the future. A clear statement that health impacts are likely underestimated due to the inability at this time to consider the other potential health effects from diesel engine exhaust constituents and secondary transformation products would be a good addition.

   CARB Staff Response: Staff has revised the discussion of health endpoints quantified and unquantified, with extensive explanation of potential health effects in the sensitivity

discussion. Staff also noted that taken as a whole, the analysis should be considered an underestimate due to the unquantified effects.

Specific comments:

1.Page ES-2, first sentence under Public Health Assessment. Suggest rewording the sentence to read "As part of the emission reduction plan, ARB staff estimated the public health impacts for some of the quantifiable adverse health effects of the goods movement system in California. That clearly indicates that more health effects are possible but not yet readily quantifiable without substantial additional review and analysis.

2. page ES-5, 2nd paragraph, fourth sentence. Should be "implementing" rather than "implementation".

3. page ES-11, second paragraph, the statewide diesel risk reduction plan was not adopted in 1991. Diesel exhaust was identified as a toxic air contaminant in 1998 and the risk reduction plan was adopted following this identification. Also, missing the word "in" before some in the last line of the first paragraph.

4.page 1-1, first paragraph line 7. Data "are" (not "is")

5. page 1-3, first line should read "…detailed in OEHHA and ARB's review of the state ozone standard."

6. page 1-5 (and elsewhere) The statement that 70% of the potential cancer risk from toxic air contaminants in California is due to diesel particulate is misleading and actually a misstatement of what was in MATES II, the origin of this figure. MATES II evaluated cancer risks for a subset of TACs, not all carcinogenic TACs. In addition, there are many more compounds in the air that are carcinogens tht do not have quantitative risk estimates. It would be more appropriate to say that About 70 percent of the potential cancer risk from a subset of common toxic air contaminants in California…" On page III-3 there is a similar sentence that needs to be reworded.

7. page 1-7 and elsewhere. There should be some discussion of the costs of lung cancer from diesel exhaust. The costs of treating cancer is very high, and although there are fewer people expected to develop lung cancer than cardiopulmonary disease, it should be mentioned.

8. page II-2, second paragraph, third sentence, not sure you can apply the Sioutas and co data to ALL components of vehicle exhaust. You can't apply it to all traffic-related pollutants, e.g., NO2 which forms from NO emitted by vehicles and is actually higher in concentration further from the freeway than right next to it.

9. page IV-6 – Should have some quantitation of the reduction of cancer in the health benefits section – it is not mentioned that I could find, but is an important endpoint.

10. Page V-1. second paragraph, 3rd sentence. Suggest rewording to "The health impacts are concentrated on nearby communities and the need for mitigation is urgent." The impacts have quite a huge footprint, and so it seems illogical to say "Highly concentrated" in nearby neighborhoods.

CARB Staff Response: Staff appreciates these specific comments and has incorporated them into the final plan.

6. Professor Constantinos Sioutas, University of Southern California

To begin with, I am a little perplexed by the notion of using "nitrates" and –or "sulfates" as the sole metric of estimating secondary products of PM from diesel sources. Depending on season, roughly 30-70% of PM2.5 organic carbon (OC) in the South Coast basin comes from secondary formation and is substantially more important from a toxicological perspective given that an abundance of studies has shown little or no toxicity for ammonium nitrate and ammonium sulfate at realistic concentration levels, whereas the opposite is true for secondary OC (Sardar et al, 2005; Schauer et al., 1996) OC has been almost entirely neglected in all of these discussions. Why is that?

CARB Staff Response: Staff has added OC (secondary organic aerosols) into the health analysis.

The impacts of PM from various sources associated with the goods shipment on public health are estimated assuming population-based exposure models and PM mass concentrations measured at single outdoor monitoring sites as surrogates of population exposures to ambient air PM. The extent to which outdoor measurements accurately reflect PM exposures has been the subject of considerable scientific debate. Results from numerous exposure studies (Cassee et al., 2005; Steerenberg et al 2004; Schlesinger and Cassee, 2003), suggest that personal PM exposures might differ substantially from outdoor concentrations due to contributions from indoor sources. Moreover, the characteristics of labile PM-bound species from outdoor sources undergo transformations as they infiltrate indoors. For example, components such as ammonium nitrate as well as semivolatile organics almost entirely volatilize in indoor environments. This has obviously enormous implications on exposure as well as in dosimetry; given that particle-phase species outdoors may become vapors-gases in an indoor environment.

CARB Staff Response: Staff acknowledges the extent of the difference between personal exposures and outdoor ambient concentrations. However, staff specifically used CR functions that relate ambient exposures to changes in health endpoints. Thus; the issue of personal exposures has minimal impact.

Major uncertainties that could be better discussed therefore include the influence of indoor exposures, the link between central site, indoor concentrations and personal exposures, and the spatial and temporal variation in concentrations of toxic PM components.

CARB Staff Response: Staff expanded the discussion of uncertainties to reflect these thoughts.

Another point that I would have liked to see addressed is related to emission inventories and the way the emission rates, in particular for PM from combustion sources, are used in the context of predicting exposure. Most of the emission rates from on- and off-road sources are based on a limited number of vehicles tested for the most part in dynamometer facilities, under very specific dilution ratios. Several recent studies pointed out substantial discrepancies between the emission rates determined with the above methodologies and those measured in real world environs (Burtscher, 2005; Kittelson et al, 2005). A large number of recent studies has shown that PM, and especially the toxicologically very important ultrafine portion, emitted from various types of engines are semi-volatile. Thus the formation processes of these particle follows a thermodynamic process that is highly non-linear in terms of its dependence on meteorological factors such as temperature and relative humidity. The discussion in the draft indicates that the used models to predict PM concentrations from emission inventories are modifications of one of form or another of a Gaussian dispersion methodology that may include chemical reaction terms, but it almost certainly does not take into consideration the particle-vapor phase partitioning. In other words, it only takes into consideration primary (or refractory) particles emitted from these sources and predicts their downwind from the source concentrations based on dilution-dispersion and possible chemical transformation.

Just to give an example of the degree to which the semi-volatile component of combustion-generated PM is affected by meteorological parameters, our own studies at the SCPCS showed that PM mass and number concentrations in the vicinity of a light duty freeway increase by 3-fold as the ambient temperature changes by 8 degrees C over the course of the same day (Kuhn et al., 2005)! These non-linearities associated with the semi-volatile nature of particles emitted for heavy and light duty engines create larger discrepancies between model predictions and actual ambient concentrations. This is a very important limitation of current models in terms of their ability to fully capture the emission spectrum of various PM sources and needs to be at a minimum acknowledged.

CARB Staff Response*: Staff appreciates the point made. CARB is in process of improving PM emission inventory by sponsoring several research projects and conducting in-house emission source testing. The results of these studies will allow staff to update the State's emission inventory with more accurate data. Also, an existing ARB research contract with University of California, Irvine will apply a comprehensive air*

quality model that is suitable to assess the impact of shipping emissions. This project is using the California Institute of Technology (CIT) atmospheric chemical transport model to simulate atmospheric dynamics in the South Coast Air Basin (SoCAB) of California. The CIT airshed model is a 3-D Eulerian gas-phase photochemical model that predicts the transport and chemical reactions of air pollutants. The CIT model is under continuous development at the University of California, Irvine in collaboration with researchers from the California Institute of Technology and other institutions. Nowadays, the original CIT gas-phase model is coupled with a three-dimensional size-resolved and chemically resolved inorganic and secondary organic aerosol (SOA) module. Furthermore, the model has incorporated state-of-the-science treatment of chlorine dynamics, updated gas-phase chemical mechanism, and improved numerical algorithms.

I list below suggestions for future long term investigations:

- Develop and-or update size-dependent chemically speciated (metals, EC/OC, PAH's, organic molecular tracers, NO_3) PM emission from various sources related to the shipment of goods
- Fully characterize ultrafine PM exposures (Indoor, Outdoor, Personal) associated with these sources;
- Develop and validate new monitoring techniques, especially portable (thus easily deployed) continuous monitors for chemical speciation for organics and metals for both source apportionment as well as health effects studies
- Using already established PM source emissions profiles and new state-of-the-art personal monitoring techniques, assess degree to which specific sources associated with the shipment of goods contribute to personal PM concentrations and overall population exposure
- Refine emission inventories and develop-validate dispersion models that take into account the semi-volatile nature of PM emitted from engines and vehicles associated with the shipment of goods.

CARB Staff Response: Staff appreciates the suggestions and will consider them in future work.

7. Professor Akula Venkatram, University of California, Riverside

The report presents results from a study, conducted by CARB, to estimate the impact of diesel particulate emissions from the ports of Los Angeles and Long Beach on surrounding communities. The study was conducted using the following steps: 1) Estimate diesel particulate matter (DPM) emissions from a variety of port activities, 2) Use these emissions as inputs to the Industrial Source Complex Model-Short Term (ISCST3) model to estimate ambient concentrations of DPM in the surrounding communities, 3) Convert these concentrations to risk levels for cancer and non-cancer health effects, and 4) Use population density information to convert risks to number of people likely to be affected by these health effects.

The second major objective of the study was to rank port related activities in terms of their impact on the surrounding communities. This ranking has allowed CARB to prioritize measures to reduce DPM emissions. CARB believes that this ranking of source impacts is more reliable than the concentration magnitudes, which are likely to be affected by inevitable uncertainties in emission estimates.

This review will focus on CARB's use of ISCST3 to estimate ambient DPM concentrations and rank the impacts of port sources on surrounding communities. CARB has assumed that because ISCST3 is a well established regulatory model, its application to this particular study requires little justification. The fact that ARB has used the model to "assess public health risk impacts of diesel PM emitted from the Roseville Railyard on nearby residential areas" does not constitute justification.

ISCST3 has been applied using meteorological information collected during 2001 at the Wilmington site located about 2 kilometers north of the port area. The report indicates that mixing heights were determined using EPA guidance although it is not specified which upper air station was used was used to derive these parameters. The dispersion parameters corresponded to the urban option in ISCST3. ARB has made reasonable assumptions about the characteristics of the sources associated with port emissions.

While this application of ISCST3 follows standard EPA guidance, the model estimates could be improved by using results from two field studies funded by CARB (See Yuan et al., 2005, see attached paper) to understand dispersion of surface and elevated releases in the Wilmington area. A conclusion from these field studies that is relevant to the current port impact study is that vertical dispersion is limited by the height of a shear generated boundary layer that is advected with the onshore flow.

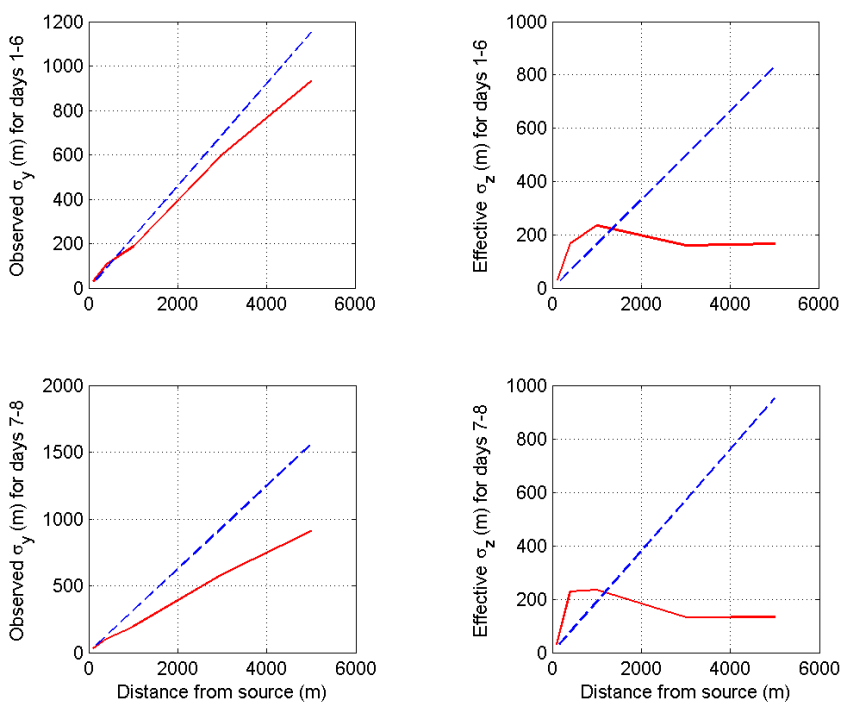


Figure 1: The variation of dispersion parameters as a function of downwind distance. The straight lines represent linear growth determined by turbulent intensities.

Figure 1, from the paper, shows that vertical dispersion is limited to about 200 m. It is unlikely that the ISCST3 dispersion curves or the mixed layer inputs would reflect this feature, which affects dispersion during onshore flows from the south; it is these flows, which occur primarily during the daytime, that bring pollutants from the port areas into the communities located to the north.

In addition to affecting the magnitudes of concentration estimates, the internal boundary layer will affect the ranking of the sources of Diesel PM. If we assume that pollutants are well mixed through the depth of this boundary layer, the long-term concentration at a receptor at a distance r from the source is given approximately by

$$\frac{C(r, z=0)}{Q} = \frac{f_\theta}{2\pi r} \frac{1}{z_i U} \qquad (1)$$

where r is the downwind distance from release, Q is the emission rate, U is the transport wind speed, z_i is the height of the internal boundary layer height, and f_θ is the relative frequency with which the wind blows towards the receptor. The relative frequency is calculated as follows. Assume that we use 8 sectors to quantify wind direction frequencies. If the probability of the wind blowing towards any sector is the same in all directions, then the absolute frequency in any one sector is 100/8 %=12.5 %. If the wind frequency in the NE sector is actually 20%, the relative frequency, f_θ, in that direction is 20/12.5=1.6.

What is important here is that the long-term concentration falls off as the distance, r, and is essentially independent of the source height if the source height is less than the internal boundary layer height. This means that the fact that the OGVs have a release height of 50 m has little bearing on the concentrations; the relative impact of a source at a receptor is governed by the source-receptor distance. Thus, not accounting for the existence of the internal boundary layer might lead to errors in both ranking of source impacts and magnitudes of concentrations.

CARB has followed EPA recommended procedures in estimating the impact of DPM sources in the ports of Los Angeles and Long Beach. However, following EPA procedures is necessary only for regulatory applications. It is clear that ISCST3 is not appropriate for estimating concentrations in this particular situation in which the internal boundary layer plays a crucial role. It is important to recall that the ISCST3 urban dispersion parameters were derived from tracer experiments conducted in downtown St. Louis in the 1960s (McElroy and Pooler, 1968), and might not be applicable to the Wilmington area.

CARB has focused on long-term concentration, which might be the most relevant variable for cancer risks. However, non-cancer health risks might be related to hourly or daily peak concentrations. It might be useful to present frequency distributions of short-term concentrations at selected receptors to assess health effects associated with short-term peak concentrations.

The report has a qualitative discussion of possible uncertainties in risk estimates. It is clearly possible to quantify these uncertainties by conducting sensitivity studies with plausible emission inventories and meteorological inputs.

Estimating concentrations associated with DPM emissions from the port areas requires in-depth understanding of the meteorology that governs dispersion. A great deal of this understanding has already been obtained through two major field studies, funded by CARB and CEC (Yuan et al., 2005). It is important to incorporate conclusions from these studies in future assessments of DPM emissions from port activities. CARB might consider using AERMOD (Cimorelli et al., 2005) in future assessments. EPA has recently proposed AERMOD as a replacement for ISCST3. AERMOD has the major advantage of being able to use on-site meteorology as inputs. For example, it can use on-site information on the internal boundary layer in estimating concentrations. CALPUFF might be useful for long-range transport studies. Note that invoking 'CALPUFF' or 'AERMOD' is not a substitute for in-depth understanding of the micrometeorology that controls dispersion.

CARB has also conducted a California wide risk assessment associated with DPM emissions. One of the steps in this assessment involved estimating the contribution of off-shore DPM emissions to total emissions from the air basin of interest. The next section provides comments on the method used by CARB to estimate this contribution.

Adjustment factors for Ship Emissions

I found it very difficult to understand the method used by CARB to estimate exposure because the description in the relevant document is too brief. Thus, my comments reflect my understanding of the method, which assumes that the basin wide averaged concentration, C, is related linearly to the corresponding emissions, Q, through

$$C = DQ, \qquad (2)$$

where D is a dispersion function, the form of which is not required in the calculations if it is assumed that it does not change with time. Then, if C and Q are known, the concentration, C_f, corresponding to projected emissions, Q_f, is

$$C_f = Q_f D = Q_f \frac{C}{Q}. \qquad (3)$$

The question that CARB addressed was: How do you include offshore emissions in Q? CARB has estimated that the total Q_T from a basin associated with offshore emissions can be expressed as

$$Q_T = Q + fQ_{off}, \qquad (4)$$

where Q_{off} is offshore emissions, and 'f 'is a fraction. CARB estimates f=0.1 for the LA Basin, and f=0.25 for the San Diego and SF basins. I found it difficult to follow the qualitative arguments that justify these choices. I am also concerned that at least the LA fraction is based on ISCST3 estimates, which I believe are not credible.

Let me suggest one way of estimating f. To do so, we need to postulate a form for the dispersion function, D, in Equation (2). The simplest equation is

$$D = \frac{1}{2\pi R_b z_i U}, \qquad (5)$$

where R_b is the radius of the air basin, and the other variables are defined in reference to Equation (1). If we take, R_b=15 km, z_i=200 m, and U=2 m/s, an emission of Q=2000 tons/year results in a basin wide averaged concentration, C= 1.5 μg/m^3.

If the contribution of offshore emissions is given by Equation (1), the fraction 'f' in Equation (4) is seen to be

$$f = \frac{f_\theta R_b}{R_{off}}, \qquad (6)$$

where f_θ is the ***relative*** frequency with which the wind blows towards the air basin, and R_{off} is the effective distance of the offshore emissions from the center of the air basin. Because $R_{off} \geq R_b$, the fraction f is likely to be less than unity if $f_\theta \approx 1$. The point here is that there is a rational method to estimate the contribution of offshore emissions to basin emissions. The qualitative arguments presented in Appendix A need to be converted to equations that others can understand.

CARB Staff Response: Professor Venkatram of UCR conducted a field study for the meteorological conditions in Wilmington site which is about 3-4 miles away from the Port of Los Angeles boundary during 7 am to 12 pm for 8 days of the period 26 August – 10 September 2004. The study concluded that vertical dispersion was limited by a shear generated boundary layer which was about 200 m. To examine if this conclusion is applied to seasonal or yearly meteorological conditions and to determine how the modeling results would change if the mixing height is capped to 200 m, we searched available meteorological measurements in the South Coast and conducted a computer modeling sensitivity study.

Previous Measurement

We contacted Mr. Lee Eddington of the US Navy who has worked with ARB on the 1997 Southern California Ozone Study and ship emissions studies. Mr. Eddington has conducted the sonde releases studies in Point Mugu and San Nicolas Island for many years. He provided us some radar propagation duct statistics for Point Mugu and San Nicolas Island. The duct is a specific terminology and it is closely related to the marine boundary layer height. Attached include four graphs which show the seasonal and mean mixing heights in Point Mugu and San Nicolas Island. In these graphs, the Optimum Coupling Height (OCH) refers to the height of the inversion base. According to Mr. Eddington's study, for most cases the OCH is equal to the mixing height. We can see that the mixing heights were about 2400 to 2600 ft (730 to 790 m) with a mean of 2560 ft (780 m) in Point Mugu (see Figures 1 and 2) and 2000 to 2500 ft (610 to 760 m) with a mean of 2380 ft (725 m) in San Nicolas Island (see Figures 3-4).

The Desert Research Institute (DRI) conducted an aircraft measurement in San Diego downtown areas. The temperature profile measured at midday on July 11, 2003 is depicted in Figure 5. This profile shows that the mixing height of the atmosphere was about 2950 ft (900 m). The annual average mixing height for 2001 in Wilmington station

was about 2260 to 2440 ft (690 to 745 m), which is the same as what we used in our modeling exercise.

Modeling Sensitivity Study

We conducted computer modeling sensitivity studies to estimate how the modeling results would change if we cap the mixing height to 200 m. We did preliminary sensitivity runs for all inland (in-port) emission sources and found that the hotelling sources are most impacted by changing the mixing height to 200 m. The scenarios for hotelling sensitivity runs are listed in Table 1. Although there are not any justifications to change the current mixing conditions which were used in our study to an arbitrary number of 200 m, for the purpose of the sensitivity study, we capped the mixing heights

to 200 m for different time periods. Case 1 considered the capped mixing height for 6 am to 6 pm every day from July 1 to September 30 (three months), and case 2 considered the mixing height for 6 am to 6 pm every day from April 1 to September 30 (half year). We compared the sensitivity modeling results with those reported in our draft report (see Table 1).

Table 1. The sensitivity study scenarios and results

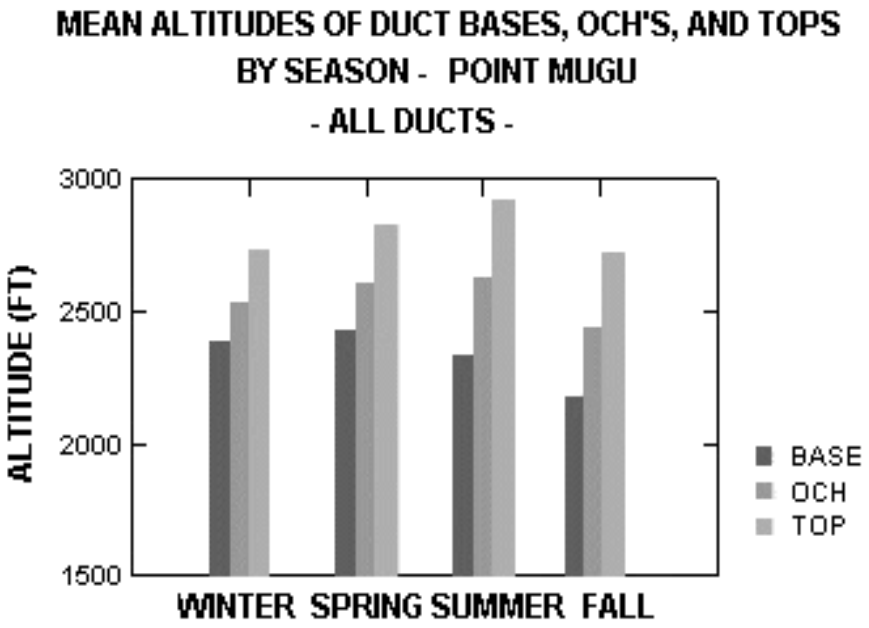
Case	*Mixing Height Scenarios*	*Hotelling*
Base case	*Actual conditions in Wilmington met data*	
Case 1	*July 1 – September 30, MH = 200 m for 6 am to 6 pm*	*11.8 % (+)*
Case 2	*April 1 to September 30, MH = 200 m for 6 am to 6 pm*	*23.0 % (+)*

As expected, the more time the mixing height is capped at 200 m, the larger impact the emission sources expose on the nearby communities. For case 1, a summer day time scenario with the mixing height being equal to 200 m, the emission impact is estimated to be about 12 % higher than what was reported in our study. As stated early, the hotelling sources are most impacted source category. The overall impact of all emission sources could probably be in the 10 percent range. Given that the estimated change is not that great and the available measurement data to support that the average mixing height is much greater than 200 m, we believe the modeling results of our study are supportable.

Conclusions

1. *Based on the annual average statistics, the mixing heights used in our modeling exercise were close to the measurements conducted by the Navy and the DRI.*
2. *The sensitivity study indicated that changing mixing height has impacts on the risks caused by diesel PM resulting from the Ports operation.*

3. *There are not any justifications to cap mixing height to 200 m. We believe that we can not arbitrarily cap the mixing height to 200 m for a seasonal or yearly time period based on the very limited observations. There are also no any reasons to do so based on the fact that what we used in our modeling exercise were close to the measurements. If we need an uncertainty estimate, we recommend a plus 10 percent.*


MEAN ALTITUDES OF DUCT BASES, OCH'S, AND TOPS
BY SEASON - POINT MUGU
- ALL DUCTS -
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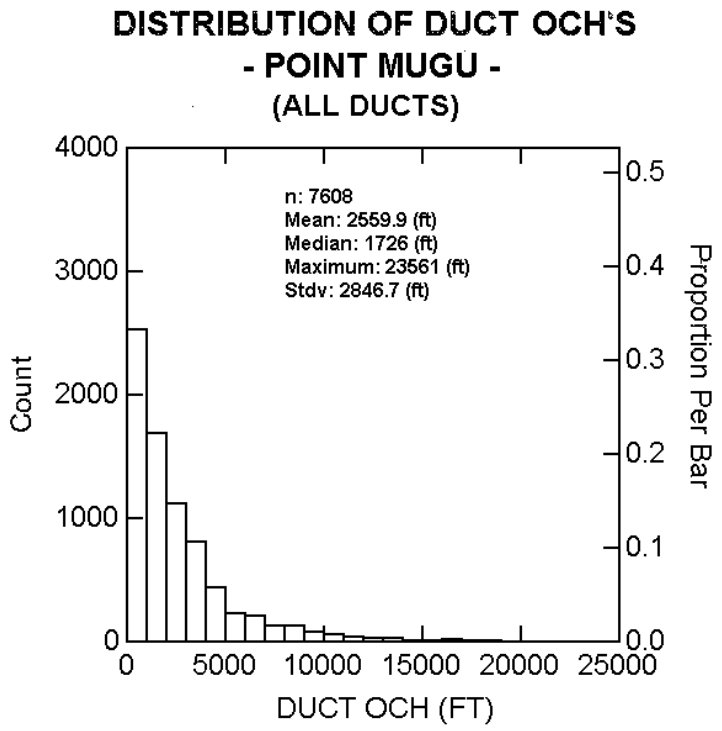
WINTER SPRING SUMMER FALL
BASE
OCH
TOP


DISTRIBUTION OF DUCT OCH'S
- POINT MUGU -
(ALL DUCTS)
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Mean: 2559.9 (ft)
Median: 1726 (ft)
Maximum: 23561 (ft)
Stdv: 2846.7 (ft)
Count
4000
3000
2000
1000
0
0
5000
10000
15000
20000
25000
DUCT OCH (FT)
Proportion Per Bar
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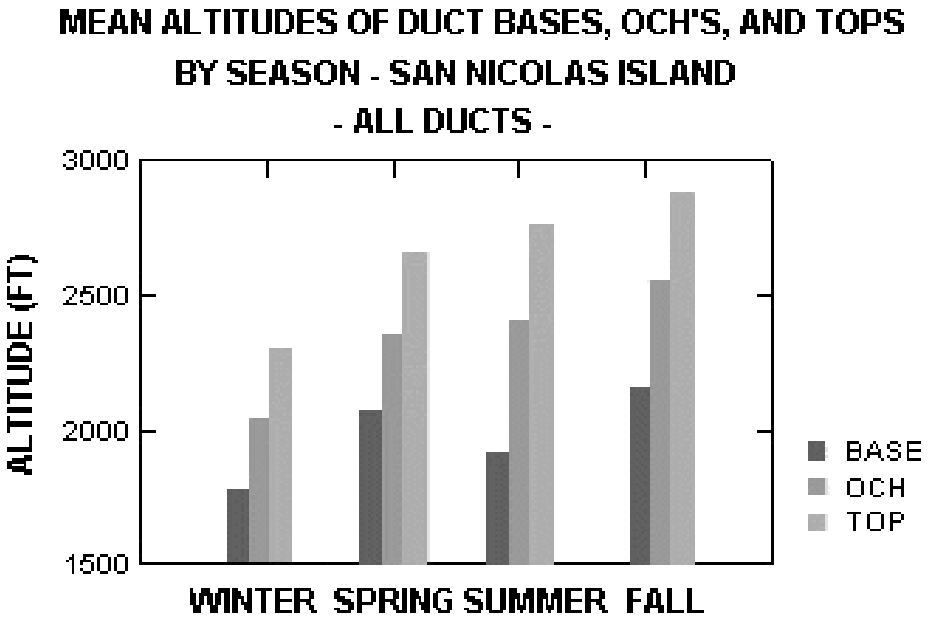
MEAN ALTITUDES OF DUCT BASES, OCH'S, AND TOPS
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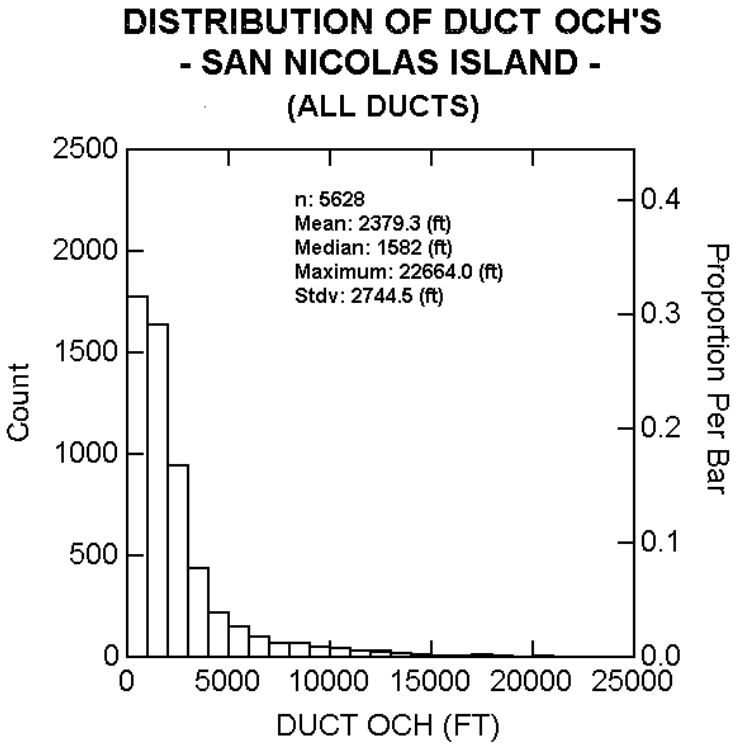
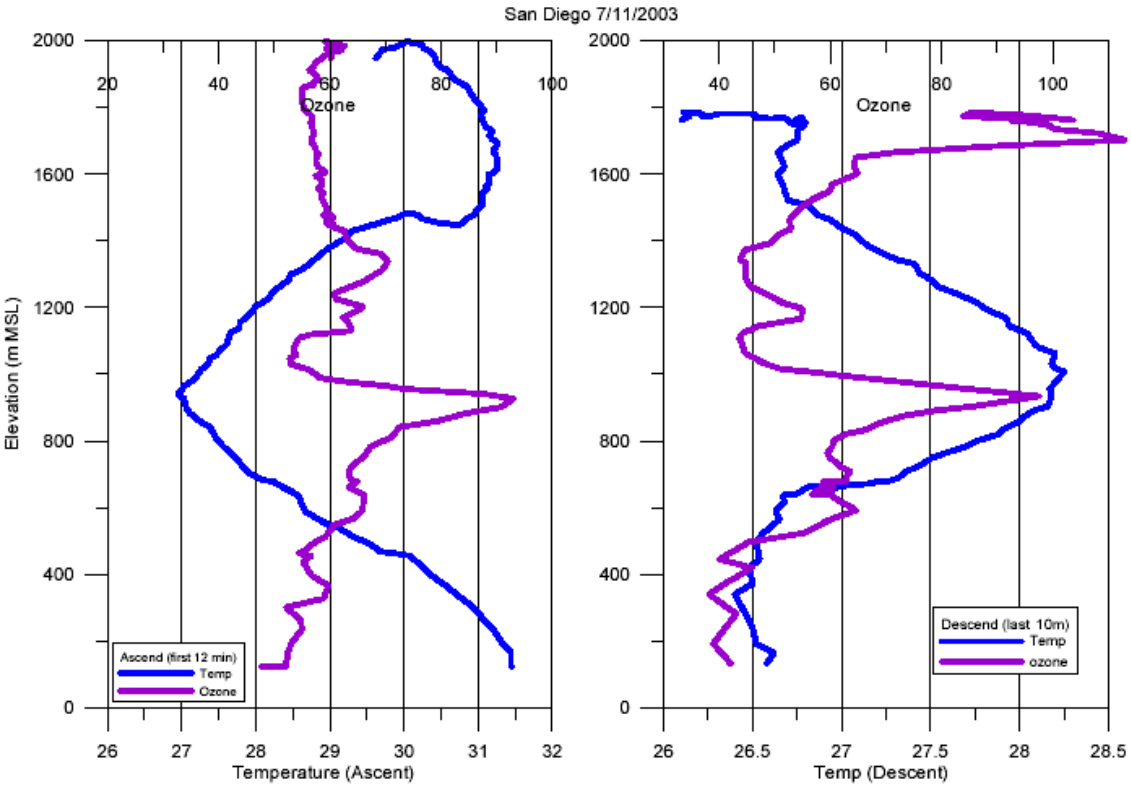

DISTRIBUTION OF DUCT OCH'S
- SAN NICOLAS ISLAND -
(ALL DUCTS)
n: 5628
Mean: 2379.3 (ft)
Median: 1582 (ft)
Maximum: 22664.0 (ft)
Stdv: 2744.5 (ft)
Count
2500
2000
1500
1000
500
0
Proportion Per Bar
0.4
0.3
0.2
0.1
0.0
0
5000
10000
15000
20000
25000
DUCT OCH (FT)



Temperature Profiles in San Diego on 7/11/2003
(Desert Research Institute)

E. Public Comments After 12/1/2005 and CARB Staff Responses

The complete list of public comments can be found in Appendix F. Below, we summarize comments that relate to the health impacts analysis and CARB staff responses (in italics).

The health risks are underestimated. The analyses should be based on work by Jerrett for mortality and Hall for school absences. ARB should use CA data.

For premature death, staff recognizes Jerrett et al. (2005) study in the Los Angeles region found a higher estimate for premature death associated with PM exposure than the national study by Pope et al., (2002), but greater uncertainty. Before results similar to Jerrett et al can be replicated elsewhere, staff could not justify applying the single-city result to other areas of California. Several additional studies have either just been published or will be in the next few months. ARB staff intends to review all of these studies and will solicit the advice of the study authors and other experts in the field and U.S. EPA to determine how to best incorporate these new results into our future assessments. For school absences at the statewide level, we used the study by Gilliland et al 2001, the same study that provided the basis for Hall's work for the South Coast Air Basin.

Effects of water pollution should be included. The number of remaining incidences of mortality and morbidity after the plan is in place should be shown.

This plan focused on the impacts of goods movement air pollution on human health. The remaining incidences of health impacts after the plan is in place are shown in the final report.

The plan should contain a more complete reference list.

The references have been updated to included all relevant literature cited within Appendix A.

The public needs more time to review the plan.

ARB has made a conscious effort to ensure adequate public participation at every step in the process, in accordance with ARB policy. Public outreach included dissemination of information through public meetings, workshop presentations, and various web pages. Public notice of the availability of the original draft plan, including the schedule for public meetings and workshops, was published on December 1, 2005, and comments were received until February 28, 2006.

The methodology is flawed and has not been adequately explained. Uncertainties were not fully explained.

The underlying methodology of the health benefits analysis has undergone rigorous peer review by the Air Quality Advisory Committee (AQAC), an independent peer review panel that was appointed by the Office of the President of the University of California. AQAC unanimously endorsed the scientific methodology. The modifications made to the methodology for this analysis were further peer-reviewed by ten experts in the field. The discussion of the methodology has been explained in more detail in the final plan. Uncertainties have also been revised to address remaining concerns of the analysis.

ARB needs to supply key input information (including examples) to allow the public to run the program.

The final Plan contains the SAS code and all information needed to run the program used in the health benefits analysis.

The methodology was not adequately peer reviewed. Peer review was rushed and of limited scope.

The underlying methodology of the health benefits analysis has undergone rigorous peer review by the Air Quality Advisory Committee (AQAC), an independent peer review panel that was appointed by the Office of the President of the University of California. AQAC unanimously endorsed the scientific methodology. The modifications made to the methodology for this analysis were further reviewed by an ad hoc review committee composed of experts in the field. The members of the review committee are listed at the following web site:

http://www.arb.ca.gov/planning/gmerp/dec1plan/health_comments/peer_review_comments/peer_review_comments.htm

Local risk should be quantified.

The methodology of the health benefits analysis is based on studies and results that are applicable to regional analyses, and cannot be readily translated into community level health assessments. Community level assessments are a high priority for the ARB and currently the Research Division of the ARB has a contract underway addressing this issue.

ARB needs to explain why there are more deaths from exposure to secondary PM than primary PM.

In both the draft and proposed plans, primary diesel PM emissions released outside of three miles from shore were adjusted to account for dispersion (described in Section III.B of Appendix A, page A-52). Without this adjustment, deaths from primary diesel PM would be greater than from secondary PM in the draft plan. Nonetheless, in the

final plan, when emissions from domestic trucks are included to address all goods movement, primary diesel PM deaths dominate the total deaths and are estimated to decrease in the future years since measures adopted by CARB are already in place to effectively reduce primary diesel PM emissions.

The plan overstates the number of deaths; also ARB needs to define a premature death. How many premature deaths/year are there in CA?

ARB staff used the health information from Pope et al 2002 study to estimate the number of deaths associated with emissions. It is the most widely cited paper that addresses PM pollution and premature death. Uncertainty ranges are presented. A premature death is defined as one that is linked to excessive exposure to air pollution. In California, about 235,000 deaths from all causes occur annually based on year 2001-2003 records from Department of Health Services. Of these, about 9,000 are premature due to exposures to pollution levels above the State air quality standards for PM and ozone.

The plans needs to include a complete health evaluation (not just air pollution) and should include factors such as noise and accidents

This plan focuses on air quality impacts. The Goods Movement Action Plan Phase II effort will address other environmental and community impacts.

The plan needs to mention the impacts on health associated with goods movement at its current level.

The final plan does mention the impacts and economic valuation of health effects associated with goods movement at the current levels. In fact, most of the Technical Supplement addresses the baseline, impacts with the existing control program.

Numerous specific edits.

We appreciate the editorial comments and have incorporated them into the final plan.

F. Scientific Peer Review Comments Prior to 12/1/2005 and CARB Staff Responses

Comments on the draft methodology document (see Section E of this Technical Supplement) sent on November 10 to scientific peer reviewers are given verbatim (except as noted in brackets) below along with CARB staff responses (in italics).

Comments from Professor John Balmes (University of California at San Francisco)

[Professor Balmes was contacted by phone and told of CARB staff's plan to use the Pope et al. (2002) associations between PM2.5 and premature death rather than Krewski et al. (2001), and to use Jerrett et al. (2005) as a sensitivity test. He concurred.]

CARB Staff Response: We proceeded as recommended.

Comments from Professor John Froines (University of California at Los Angeles)

[Addressed to Cal/EPA Secretary Lloyd – Co-signed by Professor John R. Froines (UCLA), Edward Avol, M.S. (USC), and Professor Michael Jerrett (USC).]

The purpose of this letter is to comment on the current process of developing a draft analysis for "Death and disease estimates associated with goods movement in California". I would have preferred talking with you directly but I understand you are in China with the Governor. I have appreciated your inclusion of me and other scientists in the review process; I consider participation by academic scientists to be crucial to developing the most scientifically sound document to address this major social, economic and policy issue which will have widespread ramifications for the future of California especially in the Southern California region. To develop the best response to the proposed methodology document and subsequent draft I decided that a collective effort by scientists would be most valuable and as a result I contacted Arthur Winer (UCLA), Michael Jerrett (USC), who recently published a paper on increased mortality from PM2.5 in the LA area, and Nino Kuenzli (USC), whose expertise is burden of disease analysis. We had a conference call Tuesday [November 15] to discuss the methodology document and the overall process. I have also received input from Ed Avol (USC) who has expertise regarding port emission inventories/reduction strategies, based on his efforts with the Port of LA and the NO Net Increase Task Force. Our conclusions follow:

1. Everyone expressed high regard for CARB/OEHHA scientists/professionals who are working on the analysis. We think excellent work is being conducted under difficult circumstances and respect that effort.

CARB Staff Response: One note of clarification. While we rely primarily on peer-reviewed literature reviews, analyses, and methodologies for health effects previously conducted by OEHHA scientists, the goods movement risk assessment is being conducted by CARB staff scientists with expertise in emissions, exposure, health, and economic valuation. OEHHA staff has provided internal scientific peer review for the assessment.

2. Overall, there was general dissatisfaction with the “Proposed methodology” document. Everyone expressed similar views that the document is extremely difficult to evaluate given its limited nature. The document does not provide sufficient information for an adequate scientific evaluation and overall is not clear. The four of us all have significant questions which are not addressed in the document. This is problematic and has implications for the value of the subsequent document being developed.

CARB Staff Response: The ‘Proposed Methodology’ document was a first draft written to give potential scientific peer reviewers an overview of the scope of the risk assessment, information how to access the approximately ten existing risk assessments on diesel sources and goods movement facilities already conducted by CARB staff, and planned enhancements to include pollutants, sources, and health outcomes not included in the previous analyses. We did not ask the scientific peer reviewers to pre-endorse the methodology without seeing the details and results (i.e., the methodology document is six pages versus the over one hundred pages of this document), but rather to give us as much advance notice as possible of specific concerns with our planned approach. We noted that we expected the methodology to evolve as we received comments from peer reviewers and as the analysis proceeds, which has been the case. Without specific questions, we cannot respond in more detail to this comment. However, we had contacted Professors Jerrett and Winer independently and their specific questions and comments are included and responded to below.

3. The lack of transparency of the methodology document raises serious questions about whether the analysis to be completed in about a week will be comprehensive in its content and adequately assess emissions, exposure and the anticipated health risks associated with the goods movement. Given what we have seen so far there is general concern about the potential underestimation of health risks associated with proposed goods movement policies.

CARB Staff Response: Actually, the analysis took about a dozen CARB staff three weeks (including evenings and weekends) to conduct, not one week. Again, without specific questions (as provided by Professors Jerrett and Winer), we cannot respond in more detail to this comment.

4. It is not apparent to us why there is such a tight timetable for completion of a major document that will affect the health of millions of Californians in the future. There has been major research on the health effects of air pollution conducted in California in the past decade including considerable work supported by CARB. That research has demonstrated new health outcomes at current exposure levels and reinforced our understanding of the major issues associated with exposure to air pollution in the Los Angeles Basin. There are major control and technology issues to be addressed even at current levels, and the expansion of a major transportation sector will have major implications beyond our existing concerns. A careful and thoughtful analysis of the potential human and economic consequences is required if we are to avoid adverse health consequences.

Chronic disease is difficult to measure epidemiologically and given the health endpoints including cancer, cardiovascular disease, neurological, immunological and developmental disorders, as well as allergic airway disease including asthma it will be

extremely problematic to accurately assess the true impact of expanded goods movement in coming decades on the health status of exposed populations in any timely fashion. The current approach means that we may end up assessing the death and disease, that is, the health consequences many, many years after the social/economic decisions have been made. This means that we should take the time to do the best possible job on the potential health risks and not be rushed into decisions based on incomplete information and evaluation. A longer timeframe is required.

CARB Staff Response: It is important to quantify the health effects of goods movement now (with proper acknowledgment of caveats, uncertainties, and unquantified risks) so that ongoing mitigation efforts can be based on the best available science. This risk assessment is part of an overall mitigation plan for goods movement. Waiting years or decades for new scientific findings to emerge is not an option as there are clear health and economic impacts that need to be mitigated now.

5. There was also concern that while the input of the scientific community would be included in the public record it was not apparent how the concerns we would raise would be incorporated into a final document given the timing. Inclusion of comments into the record without a commitment to modify the final document to address concerns was a matter of concern.

CARB Staff Response: As with all scientific peer reviews conducted by CARB staff, we will acknowledge and respond to all comments received into the final document.

To conclude: we believe a more deliberate process should be initiated that has a more realistic timetable and will maximize the input of the scientific community. This could include at least a one day-long meeting between members of the scientific community and scientists from CARB and OEHHA to address the wide ranging questions and methodologic issues prior to developing a draft document. I know everyone is pressed for time on this issue and, again, we respect the current efforts underway, but we also think there are too many unresolved issues at this point to develop a comprehensive document for peer review. I am available for further discussion and the other scientists would welcome a conference call to address the concerns expressed here. Finally, we have communicated with you because we think these issues require attention at the highest levels of Cal/EPA and State Government.

CARB Staff Response: We provided all the details of our risk assessment into this draft risk assessment. This includes references to the underlying literature, the computer program code, detail inputs and results, and acknowledgment of all uncertainties, assumptions, caveats, and unquantified risks of which we are aware. We have given the peer reviewers two weeks to review this document and scheduled a workshop for public input. We are also available to meet with any individuals or groups who requests and can also provide programs or conduct further calculations (i.e., sensitivity tests) as requested.

Comments from Aaron Halberg (Abt Associates Inc.)

(1) You mention that given more time and resources, a modeling-based approach would be appropriate. Longer term, you might want to talk with Bryan Hubbell at EPA about the response-surface models he has been working on - with an initial investment of

fairly significant time and resources, this approach can potentially lead to simulations of air quality models which produce remarkably accurate results essentially instantaneously.

CARB Staff Response: We contacted Bryan Hubbell of U.S. EPA as we are interested in any short- and long-term improvements to our methodology He informed us that, at this time, the response surface modeling is divided into two areas: 1) ozone modeling at 12-km grid resolution using CAMx in the Eastern U.S.; and 2) CMAQ modeling at 36-km grid resolution for the entire U.S., with outputs of PM2.5 and component species, deposition, visibility, and ozone. As a whole, the model performs very well in replicating CMAQ responsiveness to changes in precusor emissions. However, the model's predictions are good but not quite as good in California, but might be improved with additional runs they are conducting. We are also planning on conducting some focused 12km response surface modeling in some additional urban areas in the spring of 2006. When complete, these modeling results will be useful to compare to California-specific modeling being conducted for State Implementation Plans and the potential SECA request (see Section V-C)).

(2) In the exposure section, you mention that interpolation of NO_X and SO_X will be done to the census tract - this seems like it might be overkill to me, given that you are estimating the impacts of ambient exposures (people don't tend to spend all of their time in the tract in which they live, epi [epidemiology] studies tend to use county averages, etc.) and that other sources of data will be at much higher levels of spatial aggregation (e.g. regional estimates of background levels, county-level adjustment factors).

CARB Staff Response: We interpolated PM nitrates down to the census tract level to make sure of census populations in developing population-weighted exposures. However, the population-weighted exposures were developed at the county and the basin level, consistent with the higher levels of spatial aggregation used in the epidemiologic studies.

(3) In the exposure section, you mention getting uncertainty estimates using a Kriging analysis of interpolation uncertainty, while the interpolation approach used is a simple inverse-square weighting. How exactly do you plan to generate interpolation uncertainties? Do you plan to try to propogate this uncertainty through the health impact and economic benefit calculations?

CARB Staff Response: Both Kriging and simple inverse squared distance weighting schemes come with cross-validation errors that could be used as interpolation uncertainty. In this phase of the report, we have not incorporated this source of uncertainty (due to exposure estimation into our calculations).

(4) You should be careful to avoid double counting when generating your benefits estimates - in particular, when valuing premature mortality across both PM and Ozone (are you using single or multi-pollutant studies?), MRADs across both PM and Ozone (again, single or multi-pollutant studies?), Asthma Attacks (PM) and Respiratory Hospital Admissions (Ozone) (not sure if there is overlap there or not).

CARB Staff Response: The estimates associated with PM exposures were based on studies that consider PM with various other potential confounders, including ozone.

Likewise, estimates associated with ozone exposures were based on studies that consider ozone with various other potential confounders, including PM. Since the studies do not coincide, we minimized the potential chance of double-counting.

Comments from Dr. Jean Ospital (South Coast Air Quality Management District)

[Dr. Ospital was contacted by phone and told of CARB staff's plan to use the Pope et al. (2002) associations between PM2.5 and premature death rather than Krewski et al. (2001), and to use Jerrett et al. (2005) as a sensitivity test. He concurred. Dr. Ospital stated that the local community residents in the South Coast Air Basin would also be interested in the near-source diesel PM cancer risk (i.e., increased lifetime lung cancer risk per million exposed using OEHHA's upper 95th percentile unit risk factors).]

CARB Staff Response: We proceeded as recommended on the PM2.5 and premature death concentration-response functions. These premature death estimates include lung cancer deaths as discussed in Section II D. Separate diesel PM cancer risks using the OEHHA unit risk factors can only be calculated if we know the diesel PM concentration and the size of the affected population, which generally means a dispersion modeling study as there is no routine, reliable measurement method for diesel PM. The necessary modeling analyses have been conducted for diesel sources associated the Ports of Los Angeles and Long Beach, the Roseville Rail Yard, and air basin averages. These diesel cancer risks are presented in Section II C. CARB will conduct a similar modeling analysis for the Port of Oakland expected by next year. CARB will work with the 16 largest rail yards in California to perform risk assessments for those facilities over the next three years.

Comments from Professor Michael Jerrett (University of Southern California)

[The following comments refer to the No Net Increase report risk assessment (see www.portoflosangeles.org/DOC/NNI_Final_Report.pdf, beginning on page 4-23)]

I had a quick glance at the document. My first comment would that the health benefits should be based on the attached paper [Jerrett,et al., Epidemiology, 16: 1-10, 2005] (which I led, but has had substantial input from Pope, Krewski and Burnett). This paper gives direct estimates for the LA region, while the Krewski 2000 report is based on a national study where the majority of the exposure contrast comes from sulfates in the Ohio River Valley. I did the spatial analysis and much of the statistical modeling for Krewski, so I have a detailed understanding of these exposure contrasts that may not come through without reading all 298 pages and all the appendices of Krewski. The final version of the attached paper is now in print on the website (www.epidem.com). The risk estimates here are about 2-3 times higher than reported in Krewski (and given that Krewski and Pope are co-authors, the methods used are either identical or better, based on our latest understanding of the statistical methods and likely confounding effects). I anticipate that further modeling will produce even larger health effects because we have an even better exposure surface, which is ready go and will be used in a follow up where we compare effects in LA to NYC.

Bottom line: this benefits assessment underestimates the benefits. The benefits are probably two to three times greater than stated in the report. I am confident that Krewski, Burnett, Pope and all the other ACS researchers would agree that the LA

estimates are a better basis for benefits estimation in LA,. There are many other concerns I could voice about the report, including from what I can glean a vaguely defined geographic scope. Another concern is that PM from diesel is likely to be more toxic that some of the secondary components, and none of the ACS studies (Krewski, Pope, Jerrett and others) has done an direct analysis of primary diesel. If we extend our toxicology findings, we might expect the primary diesel to elicit a higher concentration-response.

CARB Staff Response: For premature death due to diesel PM2.5, the study by Pope et al. (2002), updating the original mortality estimates of the original ACS cohort study for all-cause, cardiopulmonary, and lung cancer mortality, was used to derive the concentration-response function. For this study a 6% increased risk for all-cause mortality was identified for each 10-µg/m^3 difference in fine particle concentration (Pope et al. 2002).

A California-specific study of the same mortality endpoints in relation to ambient PM2.5 has recently been published. This study (Jerrett et al., 2005) employs many methodological advances and uses the latest techniques in spatial analysis with the intent of reducing exposure misclassification. Staff and peer reviewers felt it was premature to use these new estimates to calculate statewide mortality estimates. Several arguments are put forth by Jerrett et al. (2005) to explain the larger effect estimates found in this analysis. These include: underlying differences in the subcohort; differing rates of decline in ambient PM2.5 concentration from one metropolitan area to another (in the ACS study); greater traffic exposure; meteorological or topographic differences; and, larger exposure measurement error due to heterogeneous change in air pollution levels during follow-up. The authors provide well-developed arguments against any of these factors having a significant impact on the estimates. Given the number of potential areas for differences to occur, however, and the variability of all of these parameters in different regions throughout the state, it seems reasonable not to use these estimates before confirmatory studies can be performed in different metropolitan regions. The Jerrett et al. (2005) study does suggest that intra-urban exposure gradients may be associated with higher mortality estimates than previously supposed and that these effects are closely related to traffic exposure. The authors cite confirmation of the traffic effects in a Dutch study that found a doubling of cardiopulmonary mortality for subjects living near major roads (Hoek et al. 2002). These new estimates, once confirmed, may be particularly relevant to areas experiencing higher exposures due to goods movement.

Comments from Professor Constantinos Sioutas (University of Southern California)

<u>*Emissions*</u>

[Methodology document – We have already developed goods movement emissions estimates for TOG, ROG, CO, NO_X, SO_X, PM, PM10, PM2.5, and diesel] are these data published? This is crucial information and there is not sufficient material in this report for the uninitiated reader, like myself, to figure out how this was done.

CARB Staff Response: The emissions estimates are a combination of published data and new estimates. These details are provided in Chapter II of the main report.

[Methodology document – Goods movement emissions are split into emissions associated with imports, exports, and other emissions] what are these other emissions? This is also important to mention; or is this "other" sources what is listed below.

CARB Staff Response: These details are provided in Chapter II of the main report.

<u>*Ocean-going Vessels*</u>

Emission of PM and gaseous co-pollutants? What is exactly included in these emission profiles? Is it the same information that we have for example for trucks in dyno facilities?

CARB Staff Response: Yes, both PM and gaseous pollutants are included. The analysis is for ozone and the major components of PM2.5, so the only speciation data needed is to have the direct PM, nitrate, sulfate, and VOC emissions broken out.

<u>*Trucks*</u>

Not clear to me what exactly is T4-T7.

CARB Staff Response: These are the same VMT categories as in EMFAC. T4 and T5 correspond to light heavy duty trucks, T6, corresponds to medium heavy duty trucks, and T7 corresponds to heavy-heavy duty trucks.

<u>*Trains*</u>

[Methodology document – This means that some emissions from several rail yards will be excluded from the health analysis because their activity is domestically focused.] This is also not very clear to me. How can cargo train activities be unrelated to goods movement?

CARB Staff Response: For the purposes of the Goods Movement report, locomotive emissions are included if they are directly related to international (import or export) goods movement. Locomotive emissions associated with domestic goods movement are not included in this report.

<u>*Exposure*</u>

[Protocol document – For primary and secondary diesel PM, we will use the methodology already employed in the diesel ATCMs.] How can you tell what is the fraction of diesel PM emissions that are associated with goods movement by the county-level exposure estimate? This, to me, seems such an important key statement that some methodological description would be appropriate.

CARB Staff Response: County emissions estimates for goods movement sources are combined to create air basin estimates and then applied to the air-basin-level exposure estimates to generate air-basin-level impacts.

[Protocol document – We will also develop adjustment factors for diesel PM emissions from sources (offshore ships) that are not distributed uniformly throughout the urbanized areas…] Aren't most of these sources distributed non- uniformly? Our recent studies in Long Beach show that in just 4 sites, 2 of which are CARB-AQMD monitoring sites, the spatial distribution of species such as EC, metals, OC etc is not homogeneous, with

coefficients of divergence (CODs) in the range of 0.5-0.7, and this in sites apart by just few miles! And I am referring to PM mass based species I would not even raise the issue of the enormous spatial heterogeneity of ultrafine numbers So what assumptions are made here about which sources are uniformly distributed and which ones are not, and on what information this distinction is based?)

CARB Staff Response: Exposure due to sources at Port of Los Angeles and Long Beach are estimated using the ISCST dispersion model. Direct PM emissions from ships in other regions are estimated using the procedures described in Methodology Section E.

[Protocol document – …by using results from existing offshore tracer studies…] Have these studies been published? What are they using for off shore tracers? If V [vanadium], I have my quite serious concerns on its validity.

CARB Staff Response: The offshore traces are inert gases (i.e. sulfur hexafluoride, perfluorocarbons) that are released from the ships during special studies.

[Protocol document – …and the intake fraction approach from UC Berkeley.] For the intake fraction methodology to be used here one would have to know quite accurately within these communities the spatial variability of PM and co-pollutants of interest. If the exposure levels are based on 1-2 stationary samplers in say the entire Long Beach area, I do not see how the population density can be matched to the 1-2 data points of each community. And there is of course issue such as indoor penetration and physico-chemical modification of PM and co pollutants from these sources, all of which would greatly affect the IF model's ability to provide accurate data. Does the board plan on addressing some of these issues?)

CARB Staff Response: The intake fraction approach has been dropped since the concentration-response functions are based on community-average outdoor exposure.

[Protocol document – Since almost all of the nitrates are in the fine fraction…] This is a very incorrect statement. Our 5 year Supersite data and related publications showed that about 40 - 50% of nitrate is in fact in the coarse mode and it is not sodium, but ammonium nitrate! I would be happy to forward the related papers).

CARB Staff Response: As a conservative assumption, we assumed all the nitrate was in the form of PM2.5. In term of data availability with maximum spatial resolution for both routine monitoring network and special study PM network, this study was focused on the mean annual calculation of nitrate concentrations for 1998. We believe that mixing PM2.5 and PM10 nitrate data in this study is reasonable for annual averages because most nitrates occurs in the PM2.5 fraction. This close linkage between PM10 and PM2.5 nitrate is shown by the relationship between PM10-nitrate from SSI and PM2.5 nitrate from special monitoring network, we have estimated ratio of PM10 nitrate to PM2.5 nitrate using PTEP data at six monitoring sites in southern California. In general, the annual mean fine PM-nitrate fraction at these sites was about 0.8.

[Protocol document – We will need to estimate and subtract background sulfate (from biogenic sources and long-range transport) since this can be a significant fraction of the observations.] Here again the definition of "long range" needs clarification. Do you mean transport from Long Beach to Riverside or from off shore emissions inland?

CARB Staff Response: We mean intercontinental transport.

General Comment on EXPOSURE: How does the above relate to CARB's Long Beach/LA Port report? That report was based entirely on *primary* Diesel. I think that somewhere in this document, efforts should be made to clearly delineate the steps and processes that will be taken by CARB to estimate the total fraction of $PM_{2.5}$ that is a result of goods movement from all sources, primary and secondary. I think the question of "what would air quality be without goods movement" is very important and I am not sure it can be addressed by characterizing what appear to be 2 sole markers of pollution, i.e., $PM_{2.5}$ and Ozone.

CARB Staff Response: In the Long Beach/LA Port Report, a detailed modeling approach was taken for the small 20 mile x 20 mile domain. In this report, staff determined that the entire state of California could not be modeled. Instead, we relied on emission estimates to develop the fractions of total emissions that are due to goods movement and documented the steps used to develop health impacts associated with goods movement.

Comments from Professor Arthur Winer (University of California at Los Angeles)

I did read the document over the weekend and as far as the Exposure part I have only one major concern: Whether it's appropriate to use a county level resolution for secondary air pollutants in basins like the SoCAB or Bay Area when it comes to multiplying total exposure estimates by the fraction of precursor emissions for each county. I'm not sure this will work well for secondary air pollutants for all the obvious reasons. I assume staff has thought about this or I'm being confused by the ambiguous way the discussion treats county vs. air basin.

CARB Staff Response: Based on these comments, we did all the calculations at an air basin level.

I also felt relying on CARB, 1998 and the Cass and Schaeur studies, while perhaps the best you can do, is to rely on estimates and studies that are becoming dated.

CARB Staff Response: These are just used to check the original diesel PM model estimates, which has a similar base year (1990) as the Cass and Schauer studies.

Finally, in two places in this section census "tracks" should of course be census" tracts."

CARB Staff Response: This has been fixed.

[The following comments refer to the No Net Increase report risk assessment (see www.portoflosangeles.org/DOC/NNI_Final_Report.pdf, beginning on page 4-23)]

I find inconsistencies in the way the authors of this draft treat the uncertainties in both the emissions estimates and health outcomes estimates (current and future).

I find inconsistencies in the way the authors of this draft treat the uncertainties in both the emissions estimates and health outcomes estimates (current and future). If one understands the large uncertainties that underly modeled estimates of current and future PM and NO_X emissions in any given airshed, let alone over the entire state, then

one also understands why the use of four, five and six significant figures with respect to emissions or emissions reductions estimates does not represent defensible science. Thus, the use of a number like 598,965 tpy [tons per year] for the statewide NO_X inventory is ridiculous. Similarly, quoting PM and NO_X reductions to the nearest 1 ton in Table 1 is not defensible.

To be fair, in parts of the narrative the authors do treat emissions estimates more properly, e.g. in the first paragraph using 28,000 tpy and 25,000 tons for the statewide diesel emissions inventory and PM emissions reductions estimate, respectively. What the authors need to do is go through this analysis systematically and reduce the number of sig figs [significant figures] in all cases to two, or at most three, sig figs, as appropriate.

Note, this problem of not acknowledging the uncertainty in the emissions and emissions reductions estimates has direct implications for the health outcomes estimates. Namely these also are often given to an accuracy/precision not supported by the input data used in their calculation. Again, the report is inconsistent in the way it treats significant figures for the health outcomes, in some places using two sig figs, e.g. 41,000 asthma attacks (even this should be rounded to 40,000) but in other places, e.g. in Table 2, giving mortalities to the nearest tenth of a death. Anyone who thinks we know what the avoided premature deaths in 2025 will be to the nearest tenth of a death is seriously deluded.

Personally, I'm against ever quoting a single number for these kinds of health outcomes projected far into the future. What should be given is only a range representing the 95% confidence intervals. To their credit, the authors do in many cases give the range and often to one or two significant figures, so again the report does better in some places regarding this issue than in others. But I would emphasize that Mike's [Jerrett of USC] indication the estimated benefits in this draft are too low by factors of two or three (!) is more evidence for why these authors need to be much more conservative in the way they present the data for both emissions and health outcomes (current and especially future).

Finally, the constant misuse and abuse of significant figures by the risk assessment community, failing to acknowledge the generally large uncertainties in the emissions models, exposure estimates and health outcome data, is a big part of the reason I have considerable cynicism and mistrust about the risk assessment process itself. The way many of the data in this report are presented does nothing to ameliorate my concerns.

CARB Staff Response: We agree that all uncertainties need to be acknowledged, that ranges should be presented whenever we show an central estimate, and that significant figures need to be reduced to one or two (or if we want to include in intermediate calculations so others can reproduce the final results, we should at least acknowledge that they have no meaning). Where possible, we will provide quantitative estimates of uncertainty. However, only qualitative or semi-quantitative discussions are possible for the emission and exposure estimates. To combine uncertainties for the concentration-response functions and the economic valuations, we are using a first-series Taylor series expansion.

G. Proposed Methodology – November 10, 2005 Peer Review Draft

1. Summary

California Air Resources Board (CARB) staff has been tasked to develop an estimate of the health and economic impacts caused by international goods movement as part of the California Goods Movement Report due for a public release in early December. This document represents our current thinking on methodologies that could be used. We expect this document will continue to evolve as we receive comments from peer reviewers and as the analysis proceeds.

Given more time and resources, modeling approaches using CALPUFF and/or CMAQ to estimate particulate matter (PM) and ozone concentrations associated with goods movement would be appropriate. However, given the short time frame to generate health and economic impact estimates, modeling is not an option. Thus, our exposure and health risk methodology for diesel PM and particle nitrates (Lloyd and Cackette, 2001), modified to a region-by-region approach, with the addition of similar methodologies for particle sulfates and ozone, is proposed to achieve our internal deadline (November 21). All health endpoints used in the PM and ozone standard reports (CARB and OEHHA, 2002; 2005) will be included, and annual impacts for 2005, 20010, and 2020 will be presented. An economic valuation of the health impacts will be performed using the same methods employed for airborne toxic control measures (ATCMs) by CARB (2003abc; 2004abc).

To correct for potential inconsistencies between exposure and emissions where emissions are not distributed uniformly in urban areas, we will develop adjustment factors for diesel PM emissions sources located in the outer continental shelf. This correction is assumed not to be necessary for secondary pollutant precursors (VOC, NO_X, and SO_X)

Since the health and economic impacts estimates will have large uncertainties, we propose to provide 5th and 95th percentile confidence bounds based on an integrated analysis of uncertainties in exposure estimates, human health concentration-response relationships, and the economic values. While including uncertainty due to emissions is desirable in this case, a quantitative assessment is not available. However, we will provide a qualitative description of sources of uncertainties in emissions, and how those uncertainties will affect health and economic impact estimates.

2. Emissions

We have already developed goods movement emissions estimates for TOG, ROG, CO, NO_X, SO_X, PM, PM10, PM2.5, and diesel PM. The inventory provides emissions by county, air basin, and source categories that are associated with goods movement. Goods movement emissions are split into emissions associated with imports, exports, and other emissions. The inventory also contains the following categories.

a) Ocean-Going Vessels (OGV)

The inventory contains emissions for nine vessel types. Most transit emissions occur in the outer continental shelf, which is defined as >3 miles from shore. Passenger vessels are the only category not considered related to import and export goods movement.

Emissions are allocated to imports/exports by the fraction of tonnage associated with imports and exports at each port. If data for a port was not available, we assumed 75% imports and 25% exports. We will generate all OGV emissions by county and air basin, including the outer continental shelf. Emissions will be split into hotelling (auxiliary engines at ports) and maneuvering/transit (propulsion engines).

b) Commercial Harbor Craft (CHC)

Emissions are calculated for a variety of smaller vessel types. Fishing vessels and ferryboats are included in the inventory and are not assumed related to import or export goods movement. Other categories are associated with imports and exports, which were split using the same approach above. A portion of emissions by vessel type is assigned to the outer continental shelf by county. We will generate all CHC emissions by county and air basin, including the outer continental shelf.

c) Cargo Handling Equipment (CHE)

All cargo handling equipment emissions are assumed related to import and export goods movement and were assigned using the port splits above. We will include CHE emissions.

d) Trucks (TRK)

The goods movement inventory contains all T4-T7 trucks and associated emissions from EMFAC. We have estimated vehicle miles traveled (VMT) associated with primary, secondary local, and secondary long-haul truck trips throughout California by air basin. Emissions were estimated for imports and exports for each air basin using port-specific splits for trucks originating at each port. “Other” truck emissions include port-related truck trips that are not primary or secondary trips, as well as all domestic VMT. We will generate T4-T7 truck emissions associated with imports and exports only. These represent primary and secondary trips to and from the ports.

e) Trains (RAIL)

The goods movement inventory contains all train emissions. We have estimated the fraction of rail activity associated with imports and exports (international trade) by air basin, and then applied import/export splits for each port as above. Non-import or export emissions are considered domestic rail activity. We will generate locomotive emissions associated with imports and exports only. This means that some emissions from several rail yards will be excluded from the health analysis because their activity is domestically focused.

f) Transport Refrigeration Units (TRU) and Dredgers (DREDG)

The inventory contains these sources by county. TRU emissions were first split between emissions occurring on trucks (95%) and trains (5%), and then assigned to imports/exports/other using import/export splits for trucks and trains by county. Dredgers were not associated with imports or exports. We will generate TRU emissions associated with import and exports only. These emissions will be added to truck and train emissions for the purposes of the health analysis. Dredgers will be included in the inventory, and added to the cargo handling equipment inventory for the health analysis.

Goods movement and Statewide emissions will be provided for the years 2000, 2001, 2005, 2010, 2015, 2020, and 2025, although the focus is on 2005, 2010, and 2020.

3. Exposure

For primary and secondary PM, we will use the methodology developed by CARB (Lloyd and Cackette 2001) and employed in the diesel ATCMs (CARB, 2003abc; 2004abc). One modification is that this methodology will be conducted on a region-by-region basis (county or air basin) for consistency with the benefit analyses in the PM and Ozone Standard Reports (CARB and OEHHA, 2002; 2005). For diesel PM, the air basin-specific population-weighted exposure estimates (for the appropriate year) from the Diesel Exhaust Toxic Air Contaminant (TAC) Identification Report (CARB, 1998) will be converted to a goods movement population-weighted exposure estimate by simply multiplying by the fraction of diesel PM emissions for the air basin that are associated with goods movement. We will estimate uncertainties by comparing the Diesel PM Identification Report estimates against advanced PM source apportionment studies conducted by Glen Cass and Jaime Schauer for the Children's Health Study and more recent results from the U.S. DOE-funded Gasoline/Diesel Split Study.

We will also develop adjustment factors for diesel PM emissions from sources (offshore ships) that are not distributed uniformly throughout the urbanized areas by using results from existing offshore tracer studies, CARB's recent modeling analysis for the Ports of Los Angles and Long Beach, and the intake fraction approach from UC Berkeley.

For particle nitrates, we have already developed a statewide exposure estimate using routine and special study (CADMP, CHS) PM10 and PM2.5 nitrate data, converted to ammonium nitrate. Since almost all of the nitrates are in the fine fraction, PM10 nitrate and PM2.5 nitrate measurements are treated as equivalent. Population-weighted county exposure estimates, related to all sources, will be calculated after interpolation of monitoring data to census tracts using inverse-square weighting with a 50-km limit. Similar to diesel PM, a goods movement population-weighted nitrate exposure estimate results by simply multiplying the total exposure estimates by the fraction of NO_X emissions for the county that are associated with goods movement. We will assume an adjustment factor for offshore emissions is not necessary since it takes several hours to convert NO_X to nitrate, although there is the potential for depositional loss over water. The results will be compared to our recent review of NO_X-to-nitrate observational and modeling studies. Uncertainty estimates will be based on a CARB-funded study of nitrate measurement uncertainties and a Krigging analysis of interpolation uncertainties.

For ozone (which has not been addressed in previous analyses), we have already performed a detailed population-weighted hour-by-hour exposure assessment by county, considering background and threshold levels, as part of the Ozone Standard Report (CARB and OEHHA, 2005). One important finding from a trend analysis for the South Coast Air Basin was that ozone levels have fallen by the same proportion (above global background of 40 ppb) throughout the Basin. This implies that the combined ROG-NO_X control strategy is equally effective everywhere. Thus, we will apportion ozone-related health effects to goods movement by the fraction of ROG emissions (lower bound) and NO_X emissions (upper bound) for the county that are associated with goods movement. We will assume an adjustment factor for offshore emissions is not

necessary since it takes several hours for ROG and NO_X oxidation to result in ozone accumulation.

4. Health

We will calculate total annual changes in the number of incidences of health endpoints (death and disease) associated with goods movement for base year 2005 and future years 2010 and 2020. This will be based on the peer-reviewed concentration-response relationships and base incidence rates in the health benefit analyses presented in the PM and Ozone Standard Reports (CARB and OEHHA, 2002; 2005). These estimates include 5th and 95th percentile confidence bounds. The health estimates will be calculated and presented on a statewide basis as well as by air basin and source category. The linearity of the concentration-response relationship will be demonstrated by showing ACS and Harvard Six-City results. The relative toxicity of PM components (diesel PM, nitrates, sulfates) have been investigates by Harvard (Laden et al., 2000) and in the Netherlands (Hoek et al., 2003), and these results will be summarized. Lung cancer impacts will not be considered separately as they are already included to some degree in PM premature death estimates (Pope et al., 2002). We will investigate this presumed overlap by converting OEHHA's unit risk factor for diesel PM to an odds ratio for comparison with the lung cancer findings for the American Cancer Society (ACS) cohort (Pope et al., 2002).

We will acknowledge other health issues in a more qualitative manner, including other health endpoints (e.g., asthma incidence, permanent lung function deficit), nanoparticles, PAHs/quinones, other TACs, and in-vehicle exposures (Fruin et al., 2002).

5. Economic Value

As with the ATCMs (CARB 2003abc; 2004abc), we will assign economic values to each health endpoint and apply discount rates for future years. Uncertainties in the economic values will be noted and a range of discount rates (3% and 7%) will be used. The economic valuation will be conducted and presented on a statewide basis as well as by air basin and source category.

6. Uncertainty Analysis

We will also estimate the combined uncertainty from the individual uncertainties in the exposure, heath, and economic components of the impact assessment. Because quantitative uncertainty estimates in emissions are not available, a qualitative discussion will be provided. We will also provide a robust discussion of caveats and limitations to the quantitative approaches applied in the analysis.

7. Peer Review

We will share this proposed methodology with peer reviewers from academic institutions, the Office of Environmental Health Hazard Assessment, the California Department of Health Services, and the South Coast Air Quality Management District, to allow advance notice of any concerns on their part. Reviewers will be selected for their specific expertise on the various components of the risk assessment. They will review the draft assessment before release to the general public.

8. Future Work

We will highlight ongoing and future efforts to improve the emission, exposure, health, and economic methodologies. These include ongoing studies of ship activity, air quality modeling for ports, the SECA measurement program and modeling analyses, research on the health impacts of nanoparticle and chronic ozone exposures, and valuation of cardiovascular disease.

9. References

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