

**Comments on  
Quantification of the Health and Economic Impacts of Air Pollution from Port-  
related Goods Movement and Port Activities in California  
(Appendix A of the Dec 1, 2005 Draft Emission Reduction Plan for Ports and  
International Goods Movement)**

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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.

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.
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.
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.

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.

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.
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<sub>x</sub>) 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.

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.

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.

**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.
- 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 PM<sub>2.5</sub> in California.
- 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.
- 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.

- 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 PM<sub>2.5</sub> 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.
- 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.
- 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.
- 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.
- 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.
- 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.

- 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.
- 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.

- 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.

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 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:

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 NO<sub>2</sub> as a potential source of health effects. Whether NO<sub>2</sub> 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 NO<sub>2</sub> 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 NO<sub>2</sub> 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/m<sup>3</sup> 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.