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California Air Resources Board
ATTENTION: **Research Screening Committee**
Research Division
P.O. Box 2815
Sacramento, CA 95812

Ladies and Gentlemen,

From the sidelines I have been watching the evaluation process unfold for the California Air Resources Board Research Division report titled "*Spatiotemporal Analysis of Air Pollution and Mortality in California Based on the American Cancer Society Cohort: Final Report*," Michael Jerrett et al., October 28, 2011: <http://www.arb.ca.gov/research/rsc/10-28-11/item1dfr06-332.pdf>

My hope was that the evaluation committee would do what they are supposed to do with any scientific paper: listen to criticism and insist that the authors address all of the criticism in appropriate and objective ways. *Instead just the opposite seems to have occurred.* The authors have hardly addressed ANYTHING, and the report remains essentially the same flawed report we saw on June 9, 2011. This is completely inappropriate for a paper that needs to meet scientific standards. Nothing in science is immune from criticism. This is not a faith-based exercise.

With the apparent approval of the agency staff, the authors have refused to correct or even address mistakes. This reinforces the widespread opinion that the California Air Resources Board cannot distinguish real science from an agenda-driven look-alike. *The look-alike may seem to be science, but it is not.* If you accept this report because the authors reached the conclusions you apparently paid them to reach, you will have something that will never stand up to independent scientific or legal scrutiny.

If the California Air Resources Board wants to take drastic steps to limit the use of diesel engines in California, they are better off to just do so WITHOUT recourse to a heavily flawed report. Your opponents will just show how you have perverted the course of science. That not only hurts your conclusions in this case but also your conclusions in other high profile cases like 'Cap and Trade.' Buying the results you want is a very dangerous business.

Flawed science harms everyone, from the scientists involved to every individual affected by the decisions that flow from the bad results.

Scientists are required to subscribe to the "utter honesty," spelled out by one of California's best known scientists, the late Nobel Laureate and Professor of Physics at Cal Tech, Richard Feynman. Allowing or encouraging anything less is reprehensible.

You should respect the real result in this study: **the data actually show little or no correlation of fine particulates (PM2.5) with premature deaths in California**, because either there is little or no correlation or the data and logic used are insufficient to extract the signal from the noise. A null result

is a common outcome of honest studies that seek to pin down small health effects of suspected pollutants and should not be viewed as a failure of the study. Well conducted and honest studies always add to our body of knowledge and will help others who come along later to design the next study to push detection limits still lower.

I have reviewed the June 9 version of the Jerrett report, the public hearing that you held in June to evaluate it, written comments by various individuals, and the very slightly revised October 28 version just released.

My overall impression is that the report is a massive effort at obfuscation and has little scientific value.

But who am I to say this? I am a PhD physicist, originally from the University of Chicago Laboratory for Astrophysics and Space Research, who is well versed in the basic techniques for extracting useful information from large data sets of imperfect quality. But I am neither a statistician nor an epidemiologist. I have no conflicts of interest in this matter. *No one is paying me to critique your report.*

As a physicist, my first and foremost concern is the integrity of science. Physicists traditionally defend the integrity of science against an establishment intent on misusing it for their narrow goals. A fine example of this were the Soviet nuclear physicists including Nobel Laureate Andrei Sakharov who took down the Lysenko agrarian scam that devastated Russian agriculture and biology.

In every case physicists want answers to these questions: **Have the researchers engaged in an objective process to reach the conclusions that they claim? Is their logic solid? Is their data solid?**

My initial impression of the report from the Abstract and Conclusion was favorable. The authors appeared to have done a comprehensive study, correcting for many complicating factors and identifying, in their words, “consistent and robust effects of PM2.5.” PM2.5 is jargon for 'fine particulate matter,' where the authors seem to mean fine particulates from the burning of diesel fuel in diesel engines but never really say so. There are, of course, other sources of fine particulates in California from soil dust to forest fire smoke that are likely of greater consequence than diesel smoke.

There were several disconcerting statements in the Abstract and Conclusion that hinted at the problems to be found in the main body of the paper. Under “Previous Work” they mentioned a considerable reliance on “models” to access exposure and nothing about measurements that would actually confirm the models. This is a red flag to those of us who have created and worked with models. The maxim here is that “All models are wrong. But some can be useful.” In other words, they are a fictional attempt to represent an often highly complex situation. The key word is *fictional*.

For example, when I lived in Santa Barbara, the community was faced with a prolonged drought that threatened to run the area out of drinking water. To make matters worse, the local 'expert hydrologist' had a model of the reservoir levels that was grimly wrong. I created a competing model that was so simple that everyone could understand it. It was purely statistical and contained no real physics or meteorology. The only advantage it had was the ability to vaguely reproduce the previous history of the reservoir and to suggest that the chance of Lake Cachuma going dry was 67% rather than the

0.0001 % chance claimed by the 'expert.' My model prompted a series of public policy decisions that helped the South Coast to be prepared when the lake actually did go dry briefly. Hence, the model was 'useful' but just for the purpose intended.

Under “Key Results #1” I noted the comparisons between the 'Hazard Ratios' for the California study and earlier national studies in the way that the authors intended: one essentially confirms the other. But wait a minute, that doesn't add up. We Californians (or former Californians) tend to be healthier than the national average and yet are likely exposed to more dust than the national average. The sunny and dry climate coupled with a robust economy and robust agriculture as well as numerous wild fires suggest that dust should be MORE of a problem.

Under “Key Results #2” I noted a caveat that started to make me suspicious. How could all-cause mortality be “*significantly associated with PM2.5 exposure*” if the results were sensitive to the models used? “Sensitive” made perfect sense if the basic fiction of the models was being acknowledged but no sense in connection with the strong statement about “significantly associated.” Why did they not use a more cautious and scientific term like “perhaps associated”?

Then in the next point the authors use the term “exposure predictions.” That rings all sorts of alarm bells in a study where “exposure” or “dose” must be well defined and not merely guessed. But the authors do talk about a “dense monitoring network,” so perhaps they intended to fold in real measurements of exposure for real people. Not so, I'm sorry to say.

The Abstract Figure 1 seems to bear out the thesis of this paper that fine particulates and NO2 are correlated with increased mortality throughout California. But the error bars are so large that the effect is clearly uncertain. And there is no indication as to whether or not they represent merely statistical uncertainties based on sample size or truly take into account all the uncertainties in such a complex model-based study. My guess is that the uncertainties are *far underestimated*. But even if they are just slightly underestimated, the error bars will cross 1.0 and suggest no effect at all! That suggests that words like “strongest evidence” are overblown.

By the time I got to the punch line “**We conclude that combustion-source air pollution, especially from traffic, is significantly associated with premature death in this large cohort of Californians,**” I wanted to believe the authors *but do not*.

In the sections that followed, I thought I would find answers to my questions but instead found a wall of complexity that seemed to be designed to encourage readers to skip to the Key Findings, Narrative, and Conclusion at the end. These were similar to the sections at the beginning, essentially repetitive.

When I encounter the level of complexity I found in pages 16 to 110, I am not inclined to be impressed but to wonder why such a simple issue needs that level of complexity *unless the authors are attempting a massive snow-job in lieu of solid logic and evidence*.

I have to wonder if this report is typical of so many today where the bottom line was predetermined by the funding agency, and the authors merely back-filled an impressive display of theoretical statistics and analysis.

In all legitimate studies of this nature, you need basically four things: 1) A plausible physical argument

that the hazard is related to certain causes of death and not merely associated with some other real causative factor, 2) A statistically large study group, 3) A reliable quantitative measurement of total exposure to the hazard and 4) A detailed cause of death. If the effect is small or non-existent at typical exposure levels, then you will need to account for all sorts of complicating factors.

None of this is license to repeatedly go through a massive data sorting and modeling process with ever more complex computer manipulations until you find a way to tease out a positive result. That is certainly not science. The more complexity, the more chance that systematic errors will skew the results. Statistical error bars do not begin to address the real uncertainties when complex manipulations are involved.

Faulty assumptions also bias the result. Had I been included in this study and died, the authors would have attributed my lifelong exposure to my address near Santa Barbara, a relatively clean location. They would never have realized that I spent many years living in the very dirty air of Chicago, where the winter snow turned black in a single day from all the coal soot, and our apartment perpetually backed up with coal smoke when the janitor stoked the boiler. Assuming that Californians remain in one place their entire lives is FAR from realistic.

Unsophisticated people may be impressed by statements such as:

“The integration of numerous land use, traffic, physiographic, and remotely sensed data into a rigorous mathematical model capable of estimating exposures in time and space extends the science of exposure assessment and gives results that are less prone to measurement error.”

But this really says that the authors relied on model estimates NOT real data. There was no “measurement error” because there were no real measurements of exposure for anyone in the study. Their approach is not rigorous by any stretch of the imagination. Do any of the authors understand the difference between real measurements and model estimates? It is the difference between reality and virtual reality. Models may be useful IF carefully calibrated with real data and used to interpolate only.

When I see many instances of hazard ratios less than 1.0 in the tables but rarely in the figures, I suspect that the researchers recognize that their results may have large uncertainties but are trying to discount the problems. Ratios less than 1.0 are physically unreasonable although statistically possible. They provide some of the reality checks that are missing or discounted in this research.

Buried deep in the report on Page 108 is Figure 22 which shows the many manipulations the authors tried before finding the one that *might* show a positive result for “all cause” mortality. I certainly think that the report needs to include the tables that backup the “LUR IND+5Met” model that yielded the anomalous HR = 1.08 in Figure 22. Why do these researchers include all sorts of detail about results they largely want to ignore and then “forget” to include the details of the results that they feature?

Figure 22 should be the primary exhibit of this report, replacing all the hysteria about fine particulates being “significantly associated with premature deaths” in California.

My personal bias is that there IS likely some hazard from exposure to fine particulates (PM2.5), and we are always better off to live in a clean environment. But this study does NOT give us a good quantitative indication of whether the hazard is negligible or significant at the concentrations typically

encountered in Los Angeles. That crucial question remains unanswered or has actually been answered as 'small' by this report.

If the potentially avoidable hazard of diesel smoke is largely buried in the background of other sources of fine particulates, it is exceedingly difficult to argue that focusing on diesel smoke is useful.

To answer the questions I raised at the beginning, I do not believe that the authors have engaged in an objective process to reach the conclusions that they state but have engaged in agenda-driven science. Their logic is far from clear, let alone solid. Their data base of subjects is huge but their knowledge of the important parameters of that database is far from adequate. **The crucial question of fine particulate exposure is not adequately determined.**

The other crucial question where the report completely fails (does not even venture a guess) is a physical mechanism for low levels of dust causing cardiovascular harm but not cancer. Without a mechanism, we are left to wonder if the real causative factor of any alleged “enhanced mortality” might be something else *completely removed from traffic related PM2.5* but likewise associated with a highly urbanized setting. There is no shortage of possibilities.

It may be difficult for the California Air Resources Board to understand that even if the authors of this study had proved that PM2.5 was “significantly associated with premature deaths” that would not necessarily say that a reduction in PM2.5 would improve the situation one bit. Consider that the crowing of a rooster just before dawn is “significantly associated” with the sun coming up every morning. But should you choose to have the rooster for Sunday dinner, the sun will still come up on Monday morning as it always has! Correlation is not causation.

Without any line of reasoning establishing diesel PM2.5 as the only possible causative factor for the associations claimed in this study and with those associations extremely weak at best, one can only conclude that this study fails to prove its case and should not be used as the basis for ANY decision making.

Sincerely yours,

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P.S. The problem with this report is well illustrated by a story involving the great astrophysicist and Nobel Laureate Subrahmanyan Chandrasekhar. When I was a graduate student at U. Chicago, Professor Chandrasekhar conducted a final oral exam for another PhD candidate who was a mathematical whiz. Chandra said he was impressed by the young man's abilities but wanted him to explain in simple terms what he had accomplished. When he could not explain his scientific logic in a clear fashion, Chandra flunked him. The wisdom of that decision has grown on me over the years.