Dr. C. Arden Pope, III
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Dear Dr. Pope:

My name is _____. I recently graduated from BYU with a degree in chemical engineering. As an undergraduate student, I read several articles about your many studies linking air pollution and health effects. I also viewed your forum to the BYU student body, "What Are You Breathing and Should You Inhale?", and attended a lecture on the same topic that you gave to graduate students in the chemical engineering department.

Needless to say, I have thought a lot about your findings. I appreciate the work that you have done, both in terms of providing the impetus for cleaning up the air we breathe, as well as the positive media attention it has brought to BYU. From what I understand you were under heavy scrutiny for many years because of your published findings on $PM_{2.5}$. I am glad that other researchers were willing to conduct analyses to corroborate your findings and that you stood by your work.

Once again, I have come across your studies. This time, however, as a concerned California citizen. I hope you can shed some light on the following matter.

The new regulations set in place by the California Air Resources Board (CARB) regarding diesel $PM_{2.5}$ will cost California citizens billions of dollars. These regulations threaten bankruptcy for the business of a good family friend of mine. Thousands like him will be directly affected, and ultimately millions will be indirectly affected. California's fragile economy could end up in an even more dire state.

On the other hand, poor air quality adversely affects all of us who breathe, especially those with certain pre-existing health conditions. Air pollution is certainly a negative externality with both monetary and non-monetary costs. In no way do I want to minimize the importance of air quality standards and the benefit they provide.

However, an appropriate cost-benefit analysis is necessary to be sure the standards are rational. I am not convinced the CARB has performed such an analysis.

I recognize that you do not create regulations. You advise the CARB of the science and they make the decision on how to regulate. Yet this is what is troubling me and where I hope you will shed some light.

Dr. James E. Enstrom's paper, "Fine Particulate Air Pollution and Total Mortality Among Elderly Californians, 1973-2002," provides evidence in support of the null hypothesis concerning the link between $PM_{2.5}$ and premature death in California. In a July 2008 teleconference, you revealed that it was Dr. Enstrom's paper that largely influenced your belief that no mortality effect of air pollution is a possibility. However, Dr. Enstrom's paper still has not been given the proper consideration in weighing whether or not the CARB's new regulations are truly necessary.

I have read the transcript to that teleconference and have listened to the audio recording multiple times. I understand there are imperfections with all epidemiologic studies and Dr. Enstrom's is no exception. Moreover, I do not pretend to be an epidemiologic expert—perhaps I do not understand all the nuances of why you do not give his findings more weight. Nevertheless, I have noticed some inconsistencies that I feel you need to address.

First, you offered to collaborate with Dr. Enstrom to analyze your data specific to California. Dr. Enstrom initiated contact to do so but received no response. Why not? Your collaboration would be very beneficial in providing evidence for or against your position.

Secondly, epidemiology is far from being a perfect science. The 95% confidence intervals in the studies linking $PM_{2.5}$ and health effects are large. Additionally, it appears there could be a geographic variation in the relationship. Why? As you have said, people are people whether they live in Steubenville or Los Angeles. Yet Dr. Enstrom's analysis shows the variation. Could this be evidence that the relationship really isn't that strong?

In reading Dr. Enstrom's paper, I independently shared others' concern of the cohort's advanced age. However, I would think that Dr. Enstrom's adjustments for age would limit any confounding. Furthermore, Dr. Michael Jerrett's California-specific study's results showed no effect on all-cause mortality, too. Dr. Jerrett's study did show increased cardiopulmonary, cardiovascular, and ischemic heart disease deaths, but all-cause mortality is the metric of premature deaths and all-cause mortality showed no effect. Why? If those specific causes increased as well as all-cause mortality, that would be significant. Since that is not the case, there is doubt as to the significance of the correlation. As Dr. Jonathan Samet said in the 2010 CARB Symposium, it is something that deserves further exploration.

More recently, you stated in your 2009 NEJM article that "a fundamental question remains: Do improvements in air quality result in measurable improvements in human health and longevity?" Despite citing many studies that lend support to your hypothesis, you made no mention of Dr. Enstrom's research or other findings that suggest a geographic variation. These findings offer support for the null hypothesis and would give the reader a more accurate understanding of why the question you posed is not a settled issue.

In light of this second point, I wonder if you do not adequately convey to the public and the CARB the imperfect nature of this epidemiologic research and the possibility of no effect in California. In your 2007 presentation to the citizens of Sevier County, Utah, it seems as though the causal relationship between $PM_{2.5}$ and disease is a certainty. You made no mention of Dr. Enstrom's findings or even the possibility of no effect in that presentation or in your PowerPoint presentation to the EPA last year. Why not?

In the 2008 teleconference you intimated ambivalence toward regulations, expressing interest only for the science and hoping that the regulation uses the science in a reasonable way. Yet the Sevier County presentation seemed to be aimed at persuading the citizens to prevent construction of a coal-fired power plant. Of course they were in Utah, not California, and Dr. Enstrom's findings are specific to California. But if only evidence showing a possible causal relationship of $PM_{2.5}$ and premature death is presented to the CARB, and there is no real mention of the possibility of no effect, then how can they possibly perform an adequate cost-benefit analysis?

Dr. Pope, I urge you to consider my comments and questions. This time you are on the other side of the table. Dr. Enstrom is under fire for defending his research. I am sure he doesn't want to put the public's health at risk. But the data speak. He does not want California to pay for regulations that are possibly of no benefit. If Dr. Enstrom's findings are a mere flicker of possibility that there is no causal relationship, then collaborate with him and stamp out that flicker. Or if your collaborative analysis finds otherwise, let the flame grow brighter. That is science—to investigate fully and, thus, attain truth.

I thank you for your time and eagerly await your reply.

Sincerely,