

Are there mortality co-benefits to the Clean Power Plan? It depends.

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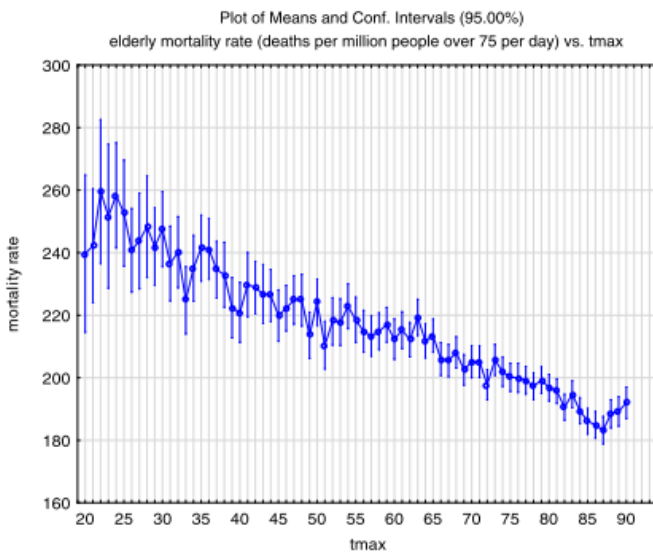
Some years ago, I listened to a series of lectures on finance. The professor would ask a rhetorical question, pause to give you some time to think, and then, more often than not, answer his question with, "It depends." Are there mortality co-benefits to the Clean Power Plan? Well, it depends.

So, rhetorically, is an increase in CO2 a bad thing? Well, for plants an increase in CO2 is a good thing. They grow faster. They convert CO2 into more food and fiber. They give off more oxygen, which is good for humans. Plants appear to be CO2 starved.

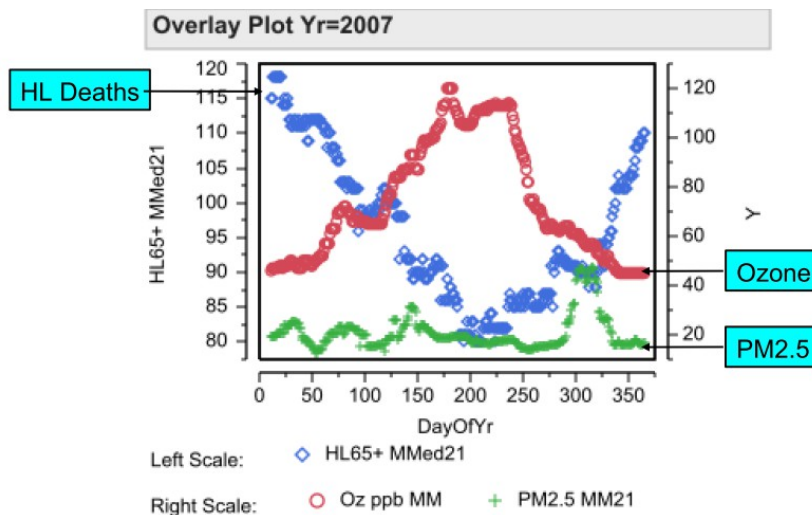
It is argued that CO2 is a greenhouse gas and an increase in CO2 will raise temperatures, ice will melt, sea levels will rise, and coastal area will flood, etc. Obviously, that senerio depends on other things as, for the last 18 years, CO2 has gone up and temperatures have not. So it depends on other factors, just what the professor said.

So suppose the temperatures do go up a few degrees. On balance, is that bad for humans? The evidence is overwhelming that warmer is better for humans. One or two examples are instructive. First, Cox et al., (2013) with the title, "Warmer is healthier: Effects on mortality rates of changes in average fine particulate matter (PM2.5) concentrations and temperatures in 100 U.S. cities." To

quote from the abstract of that paper, "Increases in average daily temperatures appear to significantly reduce average daily mortality rates, as expected from previous research." Here is their plot of daily mortality rate versus Max temperature. It is clear that as the maximum temperature in a city goes up, mortality goes down. So if the net effect of increasing CO2 is increasing temperature, there should be a dramatic reduction in deaths.



I have a very large California data set. The data covers eight air basins and the years 2000 to 2012. There are over 37,000 exposure days and over two million deaths. The data for Los Angeles for the year 2007 is typical.

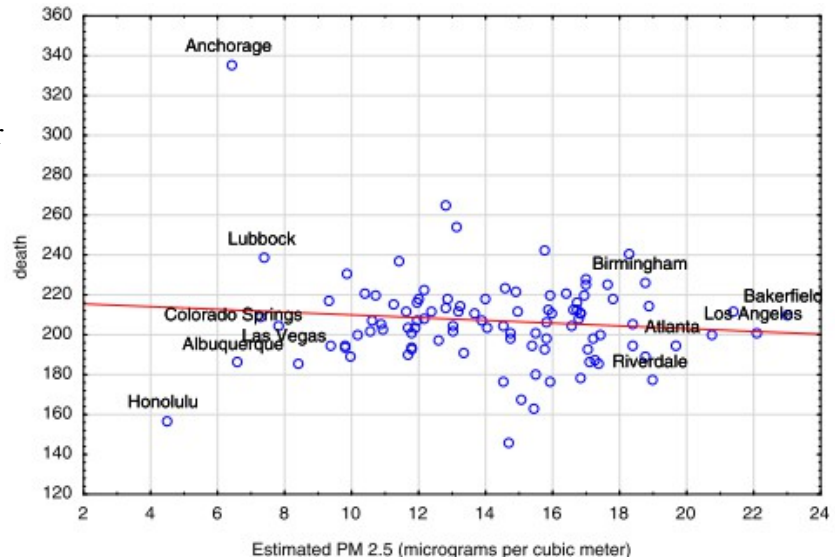


The number of Heart or Lung deaths are given on the left, y-axis. The moving 21-day median number of deaths are given with blue diamonds as time marches to the right. Deaths are high during the winter, when temperatures are lower and the number of deaths are lower during the summer, when the temperatures are higher. These plots are typical. It is well-known that higher temperatures

are associated with lower deaths.

A purported co-benefit of lower CO₂ is that there will be lower levels of PM_{2.5}. (PM_{2.5} is not chemically defined, but is partially made up of combustion products.) It is widely *believed* that lower levels of PM_{2.5} will lead to fewer deaths. Here is what Cox et al. (2013) have to say, "Unexpectedly, reductions in PM_{2.5} do not appear to cause any reductions in mortality rates." And here is their supporting figure below.

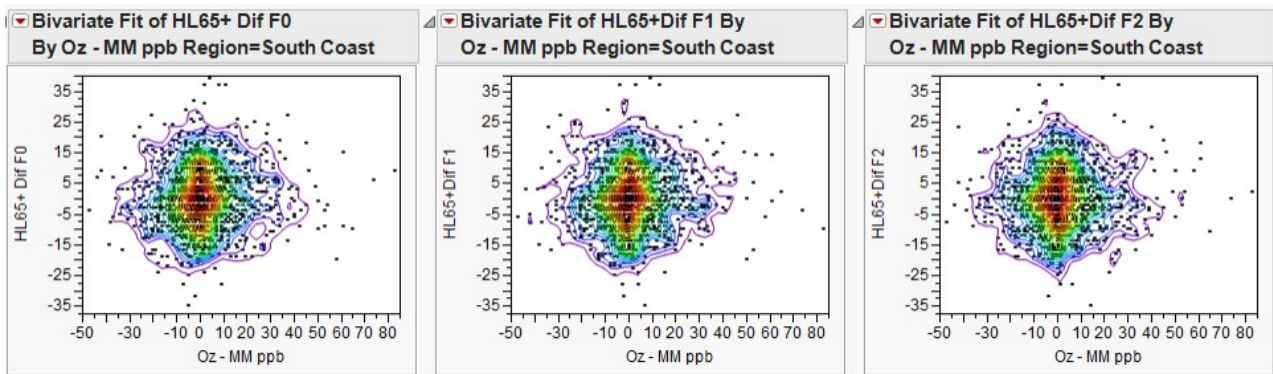
Chay et al. (2003) looked at a reduction in air pollution due to the Clean Air Act. Counties out of compliance were given stricter air pollution reduction goals. This action by the EPA created a so called natural experiment, Craig et al. (2012). The EPA selected counties did reduce air pollution levels, but there was no reduction in deaths after adjustments for covariates. Chay et al. (2003) say, "We find that regulatory status is associated with large reductions in TSPs pollution but has little



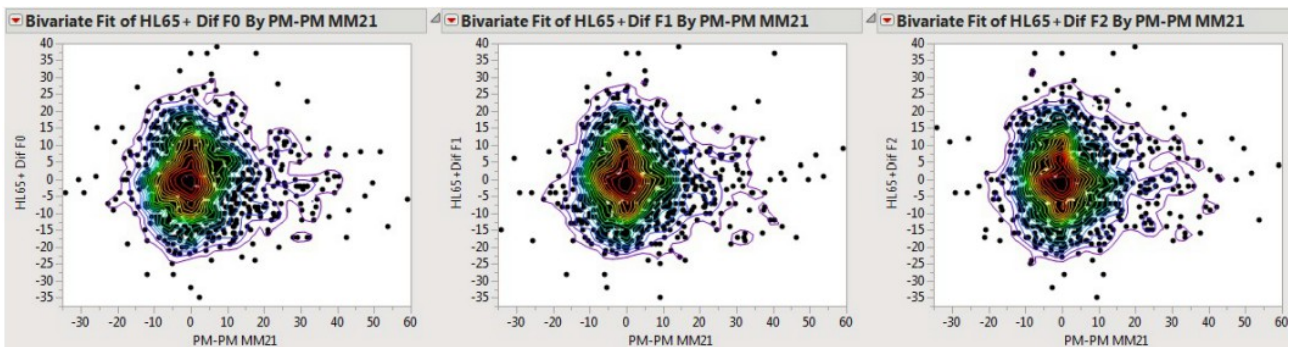
association with reductions in either adult or elderly mortality." So Cox et al. (2013) confirm the finding of Chay et al. (2003) that a reduction in PM_{2.5} does not lead to a reduction in deaths. Young and Xia (2013) find no association of PM_{2.5} with longevity in western US. Enstrom (2005) finds no association of chronic deaths with PM_{2.5} in California.

Others claim an association of air pollution with deaths. How can the two sets of claims be understood? Well, it depends. Greven et al. (2011) say in their abstract, "... we derive a Poisson regression model and estimate two regression coefficients: the "global" coefficient that measures the association between national trends in pollution and mortality; and the "local" coefficient, derived from space by time variation, that measures the association between location-specific trends in pollution and mortality adjusted by the national trends. ...Results based on the global coefficient indicate a large increase in the national life expectancy for reductions in the yearly national average of PM_{2.5}. However, this coefficient based on national trends in PM_{2.5} and mortality is likely to be confounded by other variables trending on the national level. Confounding of the local coefficient by unmeasured factors is less likely, although it cannot be ruled out. *Based on the local coefficient alone, we are not able to demonstrate any change in life expectancy for a reduction in PM_{2.5}.*" (Italics mine) In plain words, associations measured from location to location, which are likely to be affected by differences in covariates, show an association. Examination of trends within locations, which are less likely to be affected by covariates, show no association. In short, the claims made depend on how well covariates are taken into account. When they are taken into account, Chay et al. (2003), Greven et al. (2011), Cox et al. (2013), Young (2014), there is no association of air pollution with deaths.

Note well: The analysis of Young (2014) uses a moving median within a location (air basin). This analysis is much less likely to be affected by covariates. This analysis finds no association of air pollution (PM_{2.5} or ozone) with deaths. Several figures are instructive. The figures are for LA, but are typical for all California air basins. First ozone:



The figures were constructed as follows. From the daily death total was subtracted a 21-day moving median. This calculation corrects for the time trend in the data. From the daily air pollution level was subtracted the 21-day moving median for the air pollution level. The daily death "deviation" was plotted against the pollution "deviation". If air pollution was causing deaths, then the density in these three figures should go from lower left to upper right. To examine if previous air pollution, e.g. yesterday or the day before, was associated with current deaths, lags of 0, 1, and 2 days were used, hence the three figures. Plots like these were computed for all eight air basins; the figures for LA are typical. Next we give the same sort of figures, but for PM2.5. Again, LA.



Again, the density is concentrated at zero PM2.5 and zero deaths, and, the important point, there is no tilt of the density from lower left to upper right. And again the plots for LA are typical of the other seven air basins.

Can we say more? Many authors have noted "geographic heterogeneity", the measured effect of air pollution is not the same in different locations. There is overwhelming evidence for the existence of geographic heterogeneity. See for example, Krewski et al. (2000), Smith et al. (2009), Greven et al. (2011) and Young and Xia (2013). Multiple authors have not found any association of air pollution with acute deaths in California, Krewski et al. (2000), Smith et al. (2009), Young and Xia (2013) and Jarrett et al. (2013). Enstrom (2005) found no association with chronic deaths in California. A careful consideration of of this "geographic heterogeneity" is a key to understanding why it is unlikely that air pollution is *causing* deaths. Given that geographic heterogeneity exists, how should it be interpreted? First, statistical practice says that if interaction exists, then average effects are problematic. Any recommendations should be for specific situations. In the words of the finance professor, it depends. In this case it makes no sense to regulate air pollution in California more severely than current regulations.

We can consider the question of interactions of air pollution with geography more deeply? Greven et al. (2011) state in their abstract, "Based on the local coefficient alone, we are not able to demonstrate any change in life expectancy for a reduction in PM2.5." and they go on to say differences in locations (geographic heterogeneity) is most likely due to differences in covariates, e.g. age distributions, income, smoking. Indeed when Chay et al. (2003) corrected their analysis for

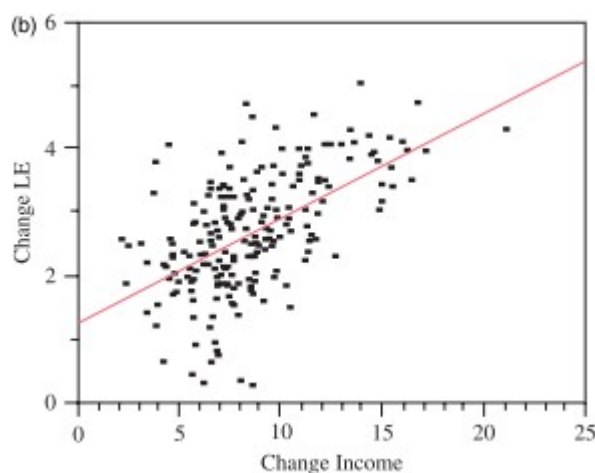
an extensive list of covariates, they found no effect of the EPA intervention to reduce air pollution.

There is empirical evidence and a logical case that air pollution is (most likely) not causally related to acute deaths. Heart attacks and stroke were recently removed as a possible etiology, Milojevic et al. (2014).

Economics on the back of an envelope

The EPA claims an excess of 6,600 deaths per year due to air pollution. If each death is valued at nine million dollars the total co-benefit is \$59.4B. But analysis that takes covariates into consideration finds no excess deaths. The \$59.4B co-benefit is the result of flawed analysis. And what is the cost of the regulation? The EPA says CPP is the most costly regulation it has considered and puts the cost at \$90B/yr. The US Chamber of Commerce puts the cost at \$270B/yr, \$900/person/year.

Consider Figure 4b of Young and Xia (2013). The data used in this figure is that used in Pope, Ezzati, and Dockery (2009) and was kindly provided by Arden Pope III. Change in income and air



pollution from ~1980 to ~2000 was used. Income in thousands of dollars increase over that time period, but differed in magnitude from city to city, the x-axis. Life expectancy increased as well, y-axis. The general trend is very clear, increased income is associated with increased life expectancy. The income-life expectancy relationship is well-known. See the dramatic video by Hans Rosling (2010). To the extent that regulations are expensive, they should move people down and left in this figure with life expectancy less than it would have been.

Summary

1. Increased CO₂ is good for plants as plants grow better with increased CO₂.
2. Increases in temperature, however caused, are good for humans as they are less likely to die.
3. The science literature, when covariates are controlled, is on the side that increased ozone and PM_{2.5} are not associated with increased deaths.
4. The costs of reducing CO₂, PM_{2.5} and ozone are expected to lead to reduced life expectancy.

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