In Response to "Clearing the Air"

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Students of law are often taught, "if you have the law on your side, pound the law; if you have the facts on your side, pound the facts; if you have neither, just pound the table." In our view, Messrs. Crandall, Rueter, and Steger have picked a fight with the wrong study. Although they raise a few good issues, their arguments are mostly just table-pounding. As the culmination of five years of careful work, with study design and independent peer review by renowned economists and scientists, the draft EPA study on the benefits and costs of the Clean Air Act makes a compelling case that Americans clearly have gotten their money's worth in terms of quantified public health and environmental benefits.

In the 1990 amendments, Congress charged the EPA with comprehensively assessing the benefits and costs of the Clean Air Act, first retrospectively and later prospectively. Further, Congress instructed the agency to establish a council of outside experts to review the data, analytical methodologies, and findings. The EPA responded by appointing a group of distinguished economists and scientists to oversee the work, chaired by former Council of Economic Advisors member Richard Schmalensee of the Massachusetts Institute of Technology. Since 1992, the council has met in open session a total of seven times, providing valuable advice to the EPA on study design and interpretation.

Crandall et al. develop several lines of attack, some of them inconsistent with one another. On the one hand, they question the finding that benefits demonstrably exceed costs; on the other hand, they suggest that even if the EPA is right that Americans have gotten a handsome return on their investment, that is not good enough because opportunities to do even better were missed.

As regards the benefits calculations, Crandall et al. suggest the EPA study overestimates emission reductions and air-quality improvements attributable to the act. They highlight a point made in the report (page 20) that because of data limitations there is a potential for overstatement of mobile source baseline emissions. They fail to note, however, the cases where baseline emissions and air-quality improvements are understated. For instance, mobile and stationary source air toxics were completely omitted, as were recreational visibility benefits. Thus, while individual components of emissions or air-quality improvements may be over- or understated, there is no basis to believe there is any systematic over- or understatement of the aggregates.

Crandall, Rueter, and Steger's assertion that the EPA study relied on early, and presumably less-reliable, emissions data to develop air-quality esti-

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mates is mistaken. Control and no-control scenario emissions inventories were constructed using an extensive array of sector models that, in turn, were calibrated using later-year, high-quality emissions inventories such as the National Acid Precipitation Assessment Program's 1985 inventory. The finding that modeled county-level emissions are not highly correlated with monitored county-level, air-quality data is hardly a serious flaw. Rather, it is a result of uncertainties in the allocation of statewide modeled emissions to individual counties and variability in other local factors. One should recall that the study looks only at the *differences* in air quality between the control and no-control scenarios, not the absolute levels. The discrepancies between modeled and monitored results have no known systematic effect on estimates of the differences between scenarios.

Contrary to the claim by Crandall et al., *direct* emissions of particulate matter (measured in the report as TSP) do not drive the benefit estimates. Certainly the authors realize that secondary particle precursors, especially sulfur dioxide and nitrogen oxides, contribute the overwhelming majority of the ambient particulate matter (PM). Curiously, their own data (see Table 3) indicate that baseline emission rates for these critical PM precursors changed little prior to 1970, thereby further undercutting their claim that emissions were overestimated.

The major concern they raise with the benefits calculations is that they allegedly overestimate premature mortality associated with PM exposure. Although Crandall et al. imply the contrary, the Clean Air Scientific Advisory Committee (CASAC) did reach agreement on the paper "Criteria Document and Staff," which states:

> "The evidence for PM related effects from epidemiologic studies is fairly strong with most studies showing increases in mortality, hospital admissions, respiratory symptoms, and pulmonary function decrements associated with several PM indices. These epidemiologic findings cannot be wholly attributed to inappropriate or incorrect statistical methods (or other methodological problems). The results provide ample reason to be concerned that there are detectable health effects attributable to PM at levels below current NAAQS."

CASAC chairman George Wolff, in reporting the panel's acceptance of the criteria document, also noted the divergence of opinion on the appropriate level of a new fine-particle standard. Crandall et al. would have you believe that because of the panel's concerns about the level at which to set a new fine-particle standard, the panel is also rejecting the broad scientific literature on PM mortality. This is incorrect. The PM-mortality relationship is well-established, although there is greater uncertainty at lower concentrations. The draft study on the benefits and costs of the Clean Air Act relied on the statistical relationships developed in CASAC-approved studies. In fact, George Wolff, a member of the Schmalensee Council, raised various PM-related issues in the course of the group's deliberations. The council considered Wolff's issues and ultimately decided to proceed with the approach described here.

Crandall, Rueter, and Steger speculate that the observed relationship between PM and mortality is really due to indoor air, because higher levels of PM occur when there is stagnant weather, which in turn reduces the exchange of indoor air to the outside, thereby causing a buildup of indoor pollution and the observed mortality increase. They fail to mention the strong longitudinal correlation between indoor and outdoor levels of fine particles, confirmed in studies conducted in twenty-four different North American cities. If the relationship was truly dependent on weather patterns, different PM/health effects would be observed in different cities. Crandall et al. are correct that no scientific consensus exists on the biological mechanism(s) by which fine particles cause premature deaths; however, this fact did not deter CASAC from reaching an agreement. In light of Congress' directive to the EPA not to adopt zero default values for scientifically uncertain effects, the lack of scientific consensus as to the underlying biological mechanism(s) should not disqualify the estimates of premature deaths.

The critique by Crandall et al. of the lead benefits analysis is also flawed. While they seem to accept the obvious and dramatic benefits to children from banning lead in gasoline, they question what the external review subcommittee described as the "well-established association between blood pressure and mortality rates" for adults over age forty-five. Indeed, the biological link between hypertension and heart disease and stroke is among the most clear-cut in medicine. Recent findings tie elevated blood-lead levels to

increased hypertension. Treatment of hypertension is not predicated on cofactors such as smoking or obesity. Further, the coefficients relating blood pressure and health effects were explicitly adjusted for age, smoking, and serum cholesterol levels. The authors' claim of exaggerated lead effects is further undermined by the study's omission (for data limitations) of potentially significant developmental and reproductive benefits.

As for the valuation of premature mortality, Crandall et al. suggest a lower value, particularly for PM, where the number of life-years shortened may be quite small. The Schmalensee Council also raised this issue, and the new draft incorporates sensitivity analyses using the age-specific-valuation and life-years-lost approaches. Not surprisingly, the benefits do decrease, but they still exceed costs by more than an order of magnitude.

As regards mobile source compliance costs, Crandall et al. are correct that the EPA used its own data to supplement the survey-based estimates from the Bureau of Economic Analysis (BEA). As is well known, however, the BEA estimates do not recognize any of the beneficial effects of pollution-control requirements on fuel economy, maintenance costs (e.g., increased longevity of spark plugs, exhaust systems, and engine oil), or improved fuel economy due to the higher energy content of unleaded gas (the cost of which is included). The EPA adjusted the BEA estimates—as it has done in "Cost of Clean" reports—and then used a macroeconomic model developed by Dale Jorgenson to simulate the effects on overall economic activity. Crandall et al. contrast the Jorgenson results with those of another model, developed by Michael Hazilla and Raymond Kopp, which used a different simulation approach and a data set from an earlier time. In effect, Crandall, Rueter, and Steger are taking sides in an ongoing modeling debate about which—it is fair to say—there is no consensus in the economic community.

There is no doubt that a marginal analysis of the benefits and costs of the act, perhaps even on a provision-by-provision basis, would have been more informative than an analysis of the total