## Air Pollution Benefit-Cost Assessment

Alan D. Krupnick and Paul R. Portney (Articles, 26 Apr., p. 522), write that pollution control costs in Los Angeles are likely to exceed benefits. This hardly qualifies as news, as some experts maintain that costs may be overstated initially (*1*) and benefits are usually understated because we know (or think we know) how to calculate only a few of them. Their main message should have been the sorry state of affairs of our knowledge of the types of effects that drive these kinds of benefit calculations.

Krupnick and Portney do not consider effects on mortality resulting from pollutants other than sulfate aerosols (*2*), effects on hospital usage (*3*), or effects on the underlying morbidity of the population (*4*). Mortality effects dominate their calculation, even though they use a value of life far less than others have recently used (*5*) and much less than the geometric mean of their own range. Increasing the value of lives prematurely lost might have balanced the control costs but would also demand that this element be examined much more closely. For example, age at death and degree of prematurity clearly merit consideration.

Ozkaynak and Thurston, the source of Krupnick and Portney's information on mortality effects (*6*), analyzed 1980 total mortality in 98 Standard Metropolitan Statistical Areas (SMSAs), using data from the Environmental Protection Agency's inhalable particle monitoring network. The sulfate measurements they used were found to be affected by artifacts from the filters (7). They concluded that their regression results were "suggestive" of an effect of particles on mortality decreasing with increasing particle size; $SO_4^{2-}$ and fine particles were significant and larger particles were not. However, when SMSAs or other large geographic areas are the observational unit in an ecological study, the finding of increased statistical significance for a variable describing submicron particles does not necessarily have health implications, because the effects of larger particles will be confined to an area closer to the measuring site and are thus not likely to be detectable by a study of large areas. Sulfates may have been selected purely on statistical grounds because estimates of their exposure are more representative of a large area (7). This constitutes an example of "effect modification," which can lead to serious bias in ecological studies (*8*). In addition, lack of a complete model specification sheds doubt on the validity of the results in general, as smoking, diet, water hardness, and migration were not accounted for (7). No other pollutants, such as ozone, $SO_2$, $NO_x$, or trace metals, were evaluated. Thus, Ozkaynak and Thurston's findings may not be very specific. Assigning the spatial mortality gradient to the wrong pollutant can have serious implications for the estimation of cost-benefit ratios, a source of uncertainty that Krupnick and Portney do not specifically consider.

Krupnick and Portney mention the uncertainties inherent in their calculations, but do not follow through to the full extent required. The problem here is not the existence of air pollution effects on mortality [which is well established (*9*)], but rather in understanding what they mean. The unstable regression results mentioned by Krupnick and Portney are most severe when sulfate particles are regressed in an incomplete model specification (7), but this does not deny the existence of the effect, it merely points to the inadequacy of the analysis.

Recent studies have shown that the phenomenon of excess daily mortality in response to peak air pollution continues downward to concentration levels well below the current standards (*10*). The magnitude of the effects found (the slope of the dose-response functions) corresponds with both the 1948–1962 London fog episodes and with cross-sectional studies that purported to find chronic effects (7). This means that a large portion of the "chronic" effect is actually the annual sum of the acute effects (*11*). However, none of these studies identifies the responsible pollutants with certainty, as the differences in reliability of exposure estimation bias the regressions in unpredictable ways. A number of time-series studies have assigned excess mortality to various pollutants, including CO, $SO_2$, NO, hydrocarbons, various forms of particles, and ozone, so it seems logical that different susceptible individuals in a large population might well respond to different pollutants. This ambiguity exists for hospital usage as well (*12*). In a sense, all the pollutants are responsible for the observed population health effects.

However, Krupnick and Portney assign all the mortality effects to sulfate particles as the responsible pollutant and state that particle acidity is the relevant property. They cite our review (*13*) of the acidity issue for substantiation. That review said (*13*, p. 1319): "the lack of appropriate exposure measurements has prevented a definitive evaluation of the association of acid aerosol with human health in any epidemiological studies completed to date" (*14*). New studies have removed part of the qualification with respect to lack of exposure information, but the statement regarding the lack of definitive epidemiology still stands. Furthermore, the levels of sulfate aerosol acidity in the Los Angeles area are modest (*15*); acidity is more likely to be found there in the gas phase ($HNO_3$) or as organic acids (*16*).

We agree with Krupnick and Portney's call for a risk-analytic approach to this problem, but the overwhelming uncertainties are not those of economics. Instead, they lie with the fragmented approach to the epidemiology of community air pollution that continues to be applied to a pressing national problem.

FREDERICK W. LIPFERT
SAMUEL C. MORRIS
*Department of Applied Science,
Brookhaven National Laboratory,
Associated Universities, Inc.,
Upton, NY 11973*

REFERENCES AND NOTES

1. W. G. Rosenberg, *Science* **251**, 1546 (1991).
2. R. H. Shumway, A. S. Azari, Y. Pawitan, *Environ. Res.* **45**, 224 (1988); P. L. Kinney and H. Ozkaynok, *ibid.* **54**, 99 (1991).
3. D. V. Bates and R. Sizto, *Can. J. Public Health* **74**, 117 (1983).
4. Because hospitalization and mortality are such severe outcomes, it follows that there must also be more frequent but less drastic health effects that have not been quantified (physicians visits, uses of medication, absence from school and work, and so forth).
5. R. L. Ottinger *et al.*, *The Environmental Costs of Electricity* (Oceana, New York, 1990), p. 99.
6. H. Ozkaynak and G. D. Thurston, *Risk Anal.* 7, 449 (1987).
7. Other cross-sectional studies have shown the importance of a complete model specification in arriving at stable regression results [F. W. Lipfert, R. G. Malone, M. L. Daum, N. R. Mendell, C-C. Yang, "A statistical study of the macroepidemiology of air pollution and total mortality" (Report 52122, Brookhaven National Laboratory for the U.S. Department of Energy, Upton, NY, 1988)].
8. S. Greenland and H. Morgenstern, *Int. J. Epidemiol.* **18**, 269 (1989).
9. The air pollution disasters of many years ago dramatically showed the existence of such effects.
10. D. Fairley, *Environ. Health Persp.* **89**, 159 (1990); J. Schwartz and D. W. Dockery, *Am. Rev. Respir. Dis.* **143** (abstract), A95 (1991).
11. J. S. Evans, P. L. Kinney, J. L. Koehler, D. W. Cooper, *J. Air Pollut. Control Assoc.* **34**, 551 (1984).
12. F. W. Lipfert, "A review of studies of the effects of air pollution on hospital admissions and use" (Report to Electric Power Research Institute, Palo Alto, CA, March 1989).
13. F. W. Lipfert, S. C. Morris, R. E. Wyzga, *Environ. Sci. Technol.* **23**, 1316 (1989).
14. This information was originally stated by P. J. Lioy and J. M. Waldman [*Environ. Health Perspect.* 79, 15 (1989)].
15. F. W. Lipfert, "Exposure to Acidic Sulfates in the Atmosphere: A Review and Assessment" (Report EA-6150, Electric Power Research Institute, Palo Alto, CA, December 1988); P. J. Lioy and J. M. Waldman, *Environ. Health Perspect.* **79**, 15 (1989).
16. B. R. Appel, E. M. Hoffer, Y. Tokiwa, E. L. Kothny, *Atm. Environ.* **16**, 589 (1982); P. A. Solomon *et al.*, "Acquisition of Acid Vapor and Aerosol Concentration Data for use in Dry Deposition Studies in the South Coast Air Basin," vol. 1 (Report No. 25, Environmental Quality Laboratory, California Institute of Technology, Pasadena, CA, 1988).

As the project director of the Office of Technology Assessment's (OTA's) study of urban ozone (*1*), cited extensively in Krupnick and Portney's article, I would like to offer a different perspective on the issue. I believe that the author's estimates of the improvements in acute symptoms (such as coughing, chest pain, asthma attacks) are the finest work on the topic. After seeing the methods Krupnick developed, OTA contracted with the authors for a nationwide assessment (*2*) and used it in our 1989 study. Where I disagree is (i) in the certainty of their "bottom line"—that the costs of controlling ozone exceeds the benefits and (ii) in the impression given by the article that a weighing of costs and benefits was absent in congressional debates over the reauthorization of the Clean Air Act.

Ironically, Krupnick and Portney's over-reliance on the strengths of their analysis—the quantification of benefits and costs—leads to the article's major weakness. Decision-makers need to consider both what we *know well* and can quantify and what we *understand only poorly* when they are weighing the benefits and costs of achieving the ozone standard. The unknowns, on both sides of the equation, are considerable.

In OTA's report, we concluded that today's technologies can achieve about two-thirds of the reductions needed to meet the ozone standard in all areas. We estimated the price tag for these controls to be about $4 to $8 billion per year in the mid-1990s and $9 to $13 billion per year a decade later. We were not able to identify the means or the costs of achieving the remaining reductions.

On the benefits side, we explained that people who are at risk of adverse effects from ozone are those with respiratory problems and those who exercise outdoors, for example, athletes, construction workers, and children at play. Each year about 20 million people are exposed to concentrations above the standard during moderate exercise, on average about 9 hours per year. On the basis of Krupnick and Kopp's analysis for OTA (*2*), we estimated that the acute symptoms, such as coughing and painful breathing, averted by reaching the standard are likely to be worth a few billion dollars per year (*3*). We emphasized, however, that medical concern centers as much—or even more—on possible damage from long-term exposure as on short-term effects, although research on chronic risks is limited and inconclusive.

Krupnick and Portney draw their bottom lines from the "known" portions of the benefit-cost comparison, that is, the costs for the two-thirds of the emissions reductions needed to reach the standard in all areas and the benefits from the selected acute health effects for which data allow such an estimate. To be fair, they do briefly mention that potential chronic health effects are not included in their estimate, but the unfamiliar reader is given no clue about the intense concern and debate over such a possibility.

Krupnick and Portney appear to lament the fact that benefit-cost assessment does not have a more prominent role in clear air decision-making. Their benefit assessment was, in fact, part of the debate over the reauthorization of the Clean Air Act, but Congress reached a different conclusion.

Krupnick and Portney's estimates are included as part of the formal justification of the bill by its primary authors, the Energy and Commerce Committee of the House of Representatives (*4*). After presenting estimates of the number of acute symptoms avoided by meeting the standard and geographically where they might accrue, the committees report (*4*) states,

> The committee believes that those are significant "gains" and as research improves, there could be more. We must achieve the present standard.

Thus the 42 members of the House Energy and Commerce Committee substituted their own judgments about the value of avoiding acute respiratory symptoms for those obtained from the economics literature. Moreover, they made judgments about the value of avoiding potential, but unproved, chronic health risks. I find this quite reasonable. The four valuation studies used by Krupnick and Portney used mail or telephone surveys of between 40 and 400 adults each, asking participants how much they would be willing to pay to avoid specific respiratory symptoms. The results of these studies provide useful information to Congress, but do we really want these studies to supersede the judgment of our elected representatives? In the end, it is the responsibility of the Congress to be the arbiter of our nation's collective values and to make the tough, yet necessary, judgment calls when our scientific and technical "crystal ball" is cloudy.

ROBERT M. FRIEDMAN
*Office of Technology Assessment,*
*U.S. Congress,*
*Washington, DC 20510*

REFERENCES AND NOTES

1. U.S. Congress, Office of Technology Assessment, *Catching Our Breath: Next Steps for Reducing Urban Ozone*, OTA-0-412 (Government Printing Office, Washington, DC, July 1989).
2. A. J. Krupnick and R. J. Kopp, "The Health and Agricultural Benefits of Reductions in Ambient Ozone in the United States" (Report to the Office of Technology Assessment, Washington, DC, June 1988).
3. Our estimate of the benefits from avoiding acute symptoms differs somewhat from those of Krupnick and Portney. Our upper bound estimates are about twice theirs because of assumptions about how to combine the results of laboratory studies of the effect of ozone with surveys of people's "willingness to pay" to avoid such effects. Our ranges for the dollar value of the selected health effects that we could quantify are (i) about $50 million to $3.4 billion per year from implementing currently available controls and (ii) about $150 million to $9.5 billion per year from meeting the standard in all areas. Benefits of "a few billion per year" from meeting the standard is a reasonable midpoint estimate based on clinical studies. When epidemiological studies are used, a midpoint estimate is closer to $0.5 billion per year.
4. Committee on Energy and Commerce, U.S. House of Representatives, *Clean Air Act Amendments of 1990, Report on H.R. 3030*; Rept. 101-490, Part 1 (Government Printing Office, Washington, DC, May 1990), p. 201.

Krupnick and Portney miss the mark in estimating the costs and benefits of cleaning up the air in the South Coast Air Basin, which includes the Los Angeles metropolitan area.

On the cost issue, the authors rely on old estimates made before the region's 20-year clean air plan was adopted in 1989. Experience in implementing the plan has since shown that the cost estimates for many of the measures were high. As the measures have been implemented, costs have fallen by almost half, largely because of rapid advances in technology, such as new and cleaner paints and materials that have eliminated the need for expensive retrofit controls. Many measures will actually save money by conserving on energy and materials.

A comprehensive cost estimate by our staff for the 1991 update of the South Coast Air Quality Management District's clean air plan estimates total costs at $6.11 billion a year, not counting $2.19 billion a year in transportation infrastructure investments. While trains, carpool lanes, and other transit facilities will improve air quality, their primary benefit will be in managing the growing traffic gridlock that is eating away at the region's economic productivity and making life miserable for many residents.

On the benefits side, the authors concentrate on only the acute health benefits of controlling ozone, basing their conclusions on a clinical study in which the response of individuals to various levels of ozone was measured in a controlled laboratory setting. To examine the benefits of reducing particulates, the authors use sulfate as an indicator.

There are serious flaws with this approach. First, people do not live in laboratories, so any estimate of benefits based on a clinical test underestimates the degree of health effects from pollution. In reality, people in the South Coast Air Basin are exposed to a broad mix of pollutants, including raw hydrocarbons, acidic particles, polycyclic organic matter, nitrogen dioxide, heavy metals, carbon monoxide, and ozone. Many of these compounds are carcinogenic, such as benzene. Second, sulfate concentrations are low in the basin because of the use of extremely low sulfur fuels, primarily natural gas in industry and low sulfur liquid fuels for transportation. Therefore, nitrate and other nitrogen-based particles are a better indicator of particulate matter in the basin, and our levels are very high. The basin is the only area in the country that still violates the federal health standard for nitrogen dioxide, which later is transformed in the atmosphere to nitrate, nitric acid, and a variety of other pollutants. Using sulfate as an indicator for estimating the health effects of particulate pollution in the basin underestimates the problem. The chief focus of plan is controlling nitrogen oxides and hydrocarbons, which contribute to the broad range of compounds in the air.

Our estimate of the health benefits of meeting the ozone standard and fine particulate standard alone is $9.4 billion a year. Additional benefits would accrue by meeting the standards for nitrogen dioxide and carbon monoxide. Because controlling emissions from the automobile and other combustion sources can reduce each of these pollutants, as well as toxic substances such as benzene, we expect that the benefits could easily double. Moreover, a recent study by Detels *et al.* (*1*) shows that exposure to the mix of pollutants prevalent in the South Coast Air Basin causes permanent loss of lung function.

What is most bothersome about the authors' argument is its stark avoidance of moral principle. I do not suspect that the authors, for instance, would tolerate anyone dumping even the most minute quantity of poison into their drinking water or food. In essence, though, that is what is happening each day in the air. And despite what the economic studies say, I defy the authors to look an asthmatic straight in the eye and tell them that their last life-threatening asthma attack could be valued at $25.

JAMES M. LENTS
*Executive Officer,*
*South Coast Air Quality*
*Management District,*
*9150 Flair Drive,*
*El Monte, CA 91731*

REFERENCES

1. R. Detels *et al.*, *Am. J. Public Health* **81**, 350 (1991).

I am intrigued by the fact that the "questioning of both healthy and infirm respondents with supplemental data on the out-of-pocket medical costs and lost income" caused by air pollution led Krupnick and Portney to assign "an average value of $25 for each asthma attack prevented" and, for nonasthmatics, "$20 for a reduction of one restricted activity day (in which an individual is neither bedridden nor forced to miss work but must alter his or her pattern in some way)."

It is understandable that economists will welcome some method—indeed, almost any method—of quantifying noneconomic goods so that they can be factored into their equations, but noneconomists may be excused for looking askance at an evaluation process that essentially equates prevention of a serious and at times life-threatening disease with allowing a jogger to carry out his daily run in comfort.

BERNARD MILLER
*Department of Chemistry,*
*University of Massachusetts,*
*Amherst, MA 01003*

The thoughtful article by Krupnick and Portney about air pollution benefits and costs seems to ignore some obvious problems, but its conclusion is sweeping: "the costs of proposed new controls are found to exceed the benefits, perhaps by a considerable margin." The study examines ozone, volatile organic compounds (VOC), and coughs. This is troubling for several reasons. First, a general view of the current major air pollution problems would rank forest health, visibility, agricultural damage, and lake acidification as greater problems than coughing. This is because U.S. successes in air pollution control have eliminated an acute health threat in much of the country. Second, ozone arises from the interaction of VOC and nitrogen oxides in sunlight. Focusing only on volatile organic compounds misleads the reader. Third, VOC and nitrogen oxides originate in the same fossil fuel combustion activities that release carbon dioxide and other pollutants. Ozone prevention policies that also reduce fossil fuel energy use are reducing the rate of accumulation of greenhouse gases.

Overall, I think the authors' conclusion is not justified by their analysis.

DUANE CHAPMAN
*Department of Agricultural Economics* and
*Program of Global Warming,*
*Institute for Social and Economic Research,*
*Cornell University,*
*Ithaca, NY 14853–7801*

*Response*: Although Lipfert and Morris take us to task about a number of points, we are in perfect agreement with what we take to be their basic point: that the epidemiology of urban air pollution is still in a confused state because of, among other things, the difficulty of isolating pollutant-specific health effects when urban air contains a heterogeneous soup of harmful substances. We focused on sulfate particles in the South Coast because many health scientists inside and outside of government are paying more attention to these particles than other particulate measures. Policy judgements will continue to be made in the absence of "definitive" epidemiological analyses, however. It seems to us that they should be informed by the current state-of-the-art, an approach we tried to follow. Lipfert and Morris also decry our omission of hospital costs from the types of benefits we valued. We omitted this category because it double counts mortality and morbidity effects and because these studies are generally unable to convincingly link specific pollutants to hospitalization.

Friedman thinks we were overly certain about our conclusions. We believe we went to great lengths to point out how national or Los Angeles ozone control benefits could be larger than we estimate and how the respective control costs could be less. We conclude that "the omitted categories would have to have large benefits associated with them, however, to tip the *apparently* unfavorable balance between benefits and costs for either . . . control plan" (emphasis added). We are comfortable with that conclusion and with the uncertainty it suggests.

Friedman also observes that Congress conducted its own implicit benefit-cost analysis in voting on the urban air quality provision in the 1990 amendments to the Clean Air Act. Bully! That's exactly how such decisions ought to be made. But if an overworked Congress is capable of making qualitative benefit-cost comparisons of air pollution measures, surely the administrator of the Environmental Protection Agency (EPA) is also. Why not allow him or her to make explicit yet qualitative trade-offs between economic, environmental, and other considerations when setting National Ambient Air Quality Standards, emission standards for hazardous air pollutants, and so on? Because these standards drive air pollution control in the United States today, the same kind of balancing Friedman attributes to Congress could enable EPA to set more sensible national environmental goals.

Lents faults us for relying on out-of-date cost estimates and ignoring the fact that some of the measures in the South Coast plan would save consumers money. With respect to the first point, it is hard to get a fix on just which South Coast plan to evaluate because it undergoes continous revisions. If we can take it as fact that the current version of the plan will cost $6.1 to $9.3 billion annually, that will make it easier for analysts to evaluate the plan in the future.

Lents appears to miss a subtle point with regard to the consequences of the South Coast plan—costs are not merely out-of-pocket expenditures. Consumer convenience carries with it a very real economic value. For example, mandatory car-pooling would reduce out-of-pocket commuting costs for gasoline and for other expenses that are shared. But there is a reason besides cheap gas and subsidized parking why freeways in Los Angeles and other urban areas are choked with single-passenger cars: people place a high value on being able to go to work when they want, come home when they want, and go to business appointments during the day at their own convenience. Giving this up would be a "cost" that would have to be reckoned into the evaluation of any regulatory program affecting consumer-commuters.

Lents comments that clinical studies always underestimate health effects in the field. In fact, the reverse is quite possible. Individuals engaged in normal day-to-day activities often can take steps to avoid actions that place them at risk from pollution. Subjects in clinical studies generally do not have this option. For instance, individuals in clinical studies are sometimes required to exercise moderately (or even heavily) while being exposed to varying concentrations of ozone or other pollutants. In everyday settings, however, many of these individuals would avoid or postpone such exercise on account of high pollution levels.

Lents is correct that nitrate particles are more prevalent than sulfates in the South Coast area. We concentrated on the latter because there is at least some epidemiological evidence linking them to premature mortality. Had convincing epidemiological studies existed for nitrates, we would have been happy to use them. On this point, we note that the benefit-cost analysis commissioned by Lent's own agency (*1*) did not use dose-response functions for nitrates either. In fact, that study used an older epidemiological analysis based on a more aggregate measure of particulates than the one we used; in any event, both studies find mortality effects of similar magnitude.

Lents also references the work of Detels *et al.* (*2*) to suggest that the mix of air pollutants found in Los Angeles may be related to permanent loss in lung function. *If* this finding is substantiated, and *if* this loss in lung function is significant enough to affect the way people live or the time at which they die, all bets are off on our estimates of the benefits of the South Coast plan. We make this clear in our article.

Lents raises "moral principles" toward the end of his letter. We leave it to readers to decide this question: at a time when so many households in Los Angeles and in the nation suffer from hunger, crime, poor health, homelessness, addiction, illiteracy, and other problems, can it *really* be wrong to ask whether the best use of society's next million, billion, or ten billion dollars lies in reducing urban ozone concentrations? That seems to us to be exactly the kind of question that we, and Lents, should be asking all the time.

Miller objects to the dollar values assigned to the improvments in human health (fewer asthma attacks) that would accompany reduced ambient ozone concentrations. Few economists are content with the valuation of reduced morbidity or premature mortality, including the empirical implementation of theoretical measures believed to be correct (*3*). We can only reemphasize the point we made in our article—that values like $25 per avoided asthma attack come from questionnaires administered to ordinary citizens, *including asthmatics*, and that these values represent average responses after mitigation measures are taken. We have no doubt that more careful extensive questioning in the future would lead to revisions in the value of avoiding acute illness and also to an improved understanding of the value of preventing chronic illness (*4*). For now, however, we can only make use of the best results available and indicate, as we did quite carefully, that uncertainties are great. Readers uncomfortable with our approach should remember that values are assigned implicitly whenever policy decisions are made; difficult as it may be, we prefer to see such assignments made explicitly and in the open.

ALAN J. KRUPNICK
PAUL R. PORTNEY
*Resources for the Future,*
*1616 P Street, NW,*
*Washington, DC 20036*

REFERENCES

1. V. Hall *et al.*, "Economic Assessment of the Health Benefits from Improvements in Air Quality in the South Coast Air Basin" (Report prepared for the South Coast Air Quality Management District, California State University and the Fullerton Foundation, Fullerton, CA, 1989).
2. R. Detels *et al.*, *Am. J. Public Health* **81**, 350 (1991).
3. M. Cropper and A. M. Freeman III, in *Health Benefits of Air Pollution Control: A Discussion* (Congressional Research Service Report for Congress, Library of Congress, Washington, DC, 1989).
4. M. Cropper and A. J. Krupnick, *J. Risk Uncertainty*, in press; *Valuing Chronic Morbidity Damages* (Report to the Environmental Protection Agency, Washington, DC, March 1989).

*Erratum*: In Kirk M. Wolter's Policy Forum "Accounting for America's uncounted and miscounted" (5 July, p. 12), there were two errors. The "Net undercount (%)" expression in the left column of page 13 should have read

Net undercount (%) = 100 × (total population − OE) ÷ total population

On page 14, equation 2 should have read

$$\hat{N} = (\hat{N}_{11} + \hat{N}_{12} + \hat{N}_{21}) + \theta \frac{\hat{N}_{12}\hat{N}_{21}}{\hat{N}_{11}}$$