Letters

We welcome letters from readers, particularly commentaries that reflect upon or take issue with material we have published. The writer's name, affiliation, address, and telephone number should be included. Because of space limitations, letters are subject to abridgment.

Risk Assessment at OSHA

TO THE EDITOR:

We have read with interest the two articles in your November/December issue on regulating risk: "The Perils of Prudence" by Albert L. Nichols and Richard J. Zeckhauser, and "A Review of the Record" by John F. Morrall III. Our comments and concerns with some of the points raised by the authors follow.

First, OSHA's procedures for risk assessment have relied on the best studies and on the best estimates of risk, not on upper bound estimates as implied in both articles. Nichols and Zeckhauser suggest that the potential upward bias in risk assessment can be averted by obtaining subjective probability estimates from a number of experts. Although this approach seems reasonable, in practice many steps in the risk assessment process are uncertain. Assigning such subjective probabilities to multiple estimates of risk may provide little improvement. Moreover, the authors' own example indicates that this approach may raise rather than reduce a best estimate of risk. Thus, the question of whether the assignment of probabilities would lead to better policy decisions remains moot.

In the article by Morrall, there is much to like and agree with. Namely we have by now sufficient history and experience in the application of economic analysis to regulatory issues to make the broad, cross-agency review of initiatives a productive and informative exercise. The technique of simply matching cost data with lives saved permits us to escape from the "no-win" dilemma of selecting a price tag for a human life. This last point is not meant to discredit the pioneering work which is reflected in the "willingness-to-pay" approach to valuing risk, but to recognize that dollar results which reflect simplified assumptions and which are based upon skimpy data bases should be viewed cautiously and as first steps in an evolutionary process.

Points of disagreement with the Morrall article concern primarily technical aspects of the analysis which have the effect of grossly distorting or exaggerating the cost of OSHA rulemaking actions. The use of cost discounting, for example, misrepresents the cost of regulations to employers. A fairer treatment would be to amortize capital costs and combine these with undiscounted recurring costs of regulation. This eliminates the assumption that cash reserves are available today to meet the cost of new rules now or sometime in the future. Discounting benefits inevitably leads to the conclusion that safety rules are more cost-effective than health rules. This is true because of the latency period associated with many diseases versus the immediate benefits realized when an accident is prevented. This conclusion, however, should be sufficient warning that something is wrong with our method. The search for a suitable method for equating safety and health rules and evaluating alternative strategies to save lives today or in the future, needs to continue. But letting the market discount rate ticker run down over the latency periods for many diseases is inappropriate, and will lead to bad policy decisions.

The astounding cost per life saved that Morrall attributes to OSHA's proposed formaldehyde standard can be produced and understood only if one appreciates the economist's desire to weigh all the evidence, epidemiological and animal studies with both positive and negative results. Unfortunately many medical scientists would argue that this approach is wrong—that studies and tests which produce positive results must be considered qualitatively different from other evidence. A dilution problem results when positive and negative test results are combined. However small projected dollar costs of a rule may be, they can appear huge when a diluted fraction is used as the risk assessment denominator and no time constraint is assumed. The result in the case of formaldehyde is a cost per life saved approximately equal to the gross national product of Denmark.

Hugh Conway
Director of Regulatory Analysis
Occupational Safety and Health Administration
Washington, DC

NICHOLS AND ZECKHAUSER respond:

Mr. Conway asserts that OSHA strives for "best estimates" in its risk assessments, rather than following the conservative procedures of EPA, which were the primary focus of our article. We cannot make a detailed comparison of the two agencies, since OSHA's track record on risk assessment is much shorter than EPA's and its procedures less carefully codified. OSHA's procedures may be somewhat less conservative than EPA's (for example, OSHA reports the maximum likelihood estimates along with the upper confidence limit—an action we applaud). Our understanding, however, is that OSHA adopts conservative assumptions at many key junctures, including the basic assumption that risk is proportional to dose at low risks. OSHA's major promulgation in recent years, the Hazard Communication Standard, was criticized by OMB for using excessive risk estimates. The study of this debate commissioned by the Secretary of Labor concluded that OSHA's estimates were overstated by roughly an order of magnitude.

We agree—and acknowledge in our article—that assigning subjective probabilities would be difficult and could pose a variety of problems. As we wrote, "the expected value approach . . . is a goal for long-range reform rather than a method that could be applied right away." In the shorter term, as we stressed, we believe agencies should focus on the more obvious sources of upward bias, which could be remedied without requiring a full-blown probabilistic assessment.

As Conway observes, expected-value estimates will often be higher than "best estimates"—but that does not constitute a valid criticism of the expected-value approach. The goal in designing risk-assessment procedures is not to minimize estimated risks, but to pursue realism and accuracy. Our argument is analytical and scientific, not political. Even if we were primarily concerned about the excesses of past risk assessment practices, we would not recommend that new procedures be tipped toward underestimation. We support the use of expected value for the reasons laid out in our article—because it is consistent with principles of rational decision making, not because it will produce low estimates of risk.

Albert L. Nichols
Richard J. Zeckhauser
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MORRALL responds:

Mr. Conway argues that OSHA's procedures for risk assessment have relied on best estimates, not upper-bound estimates as implied in both articles. But he then goes on to defend the very practices that cause OSHA's estimates to be biased. First, he does not believe in discounting benefits over the latency periods of diseases, apparently because discounting leads to conclusions he does not like, rather than because of any methodological flaw in the procedure. Besides being well grounded in economic theory, accounting for the time-incidence of health effects is simply common sense. How many more people would quit smoking if they thought that a puff of a cigarette could kill them instantly rather than 30 to 40 years into the future?

Conway gives away his point most clearly when he criticizes "the economist's desire to weigh all the evidence" in favor of what he asserts is the medical scientist's approach of using positive studies while excluding negative studies to estimate chemical risks. OSHA's formaldehyde risk assessment cited by Conway is a case in point. The animal study upon which OSHA based its risk estimates for humans had two components: a rat experiment and a mouse experiment. The results from the rat study indicated that formaldehyde was carcinogenic for rats at the highest dose. The mouse results were not statistically significant. OSHA based its risk estimates only on the rat data even though there is no consensus as to which animal is a better predictor of human risks. Regardless of whether one thinks this selective use of data is a good idea, it is a mathematical fact that the resulting estimate is thereby biased upwards—and that is true no matter what field of scientific endeavor one is working in. This and other sources of upward bias in OSHA's risk estimates for formaldehyde are elaborated in a March 22, 1986, filing submitted to OSHA's public docket by the Office of Management and Budget.

The fact that the cost per life saved from the regulation of formaldehyde approaches the gross national product of Denmark by itself does not indicate that there is something rotten in the analysis. It may instead be telling about the consequences of overzealous regulation.

John F. Morrall III
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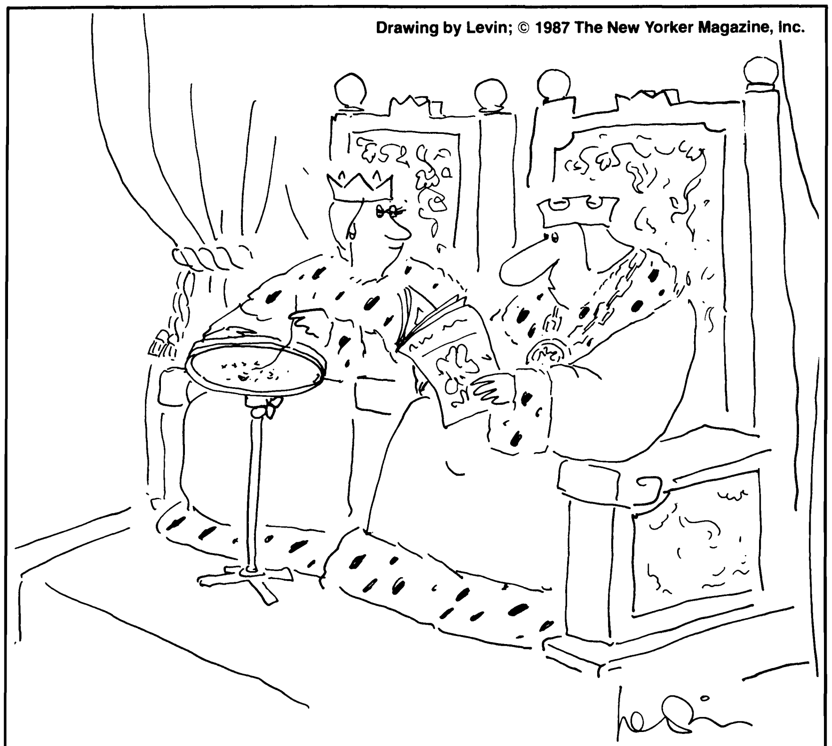
High-Priced Natural Gas

TO THE EDITOR:

William Niskanen's article, "An Alternative Perspective on the Effects of Natural Gas Regulation" (*Regulation* November/December 1986) tackles an important question: Have natural gas pipelines been buying significant quantities of gas



"Tell me, do we subscribe to this magazine, or do they just send it to us because we're the King and Queen?"

TO OUR ROYAL LEADERS: We mean our loyal readers, of course, who have patiently seen *Regulation* through one, and now another, deadline-defying transition. With this issue, the American Enterprise Institute for Public Policy Research resumes publication of *Regulation* under its traditional subtitle: AEI Journal on Government and Society.

at prices higher than they can expect to sell them at? His answer, based on various empirical tests, is no. Unfortunately, however, the empirical tests are unpersuasive.

The specific issue Niskanen addresses is whether the wellhead price of (now largely unregulated) "new" natural gas has been influenced by the presence of cheap regulated "old" gas. If pipelines sell gas at average cost due to regulatory constraints, then they are willing to overpay for some fraction of their input, and the price (and cost) of new gas probably has exceeded its marginal value to consumers. This is the conventional view. In Niskanen's alternative, pipelines evade regulatory constraints and pay prices for new gas that simply reflect their expectations of its future scarcity and hence its value.

The subject is interesting, Niskanen's alternative hypothesis is certainly plausible, at least to economists, and he seeks to bring relevant data to bear. My problem is with the tests he conducts and what they appear to show. I will discuss each of his tests in turn.

The first test graphs a time series of average price ratios between oil products and competing (retail) natural gas. According to the paper, an increase in oil prices should increase these ratios if there is average-cost pricing, whereas they will be stable if not. Niskanen argues the ratios are roughly stable in that they return to previous levels some time after each of the oil shocks. First, it is debatable whether this is or is not stability. But more important, the evidence is at best ambiguous in that it could be consistent with either hypothesis. If pipelines price at average cost, then when oil prices rise they pay even more for new gas until the price they sell at (based on the rolled-in cost) eventually becomes competitive with oil. Nothing in the graph tells us that they have not paid for and priced gas in this manner.

The second test is stronger in that it gets more to the heart of the issue, the price paid for new gas. Unfortunately, it is unclear that the test can do what is claimed for it. The regression results indicate that the variation in new gas prices paid among pipelines is less than what would be expected if old-gas "cushions" played the role implied in the average-cost pricing model. But how can prices paid by different pipelines for new gas vary at all? Aren't they competing in the same markets? A better test might look at *volumes* of new gas as related to old-gas cushions but, as is noted in the paper, there may be other motives inducing pipelines to acquire higher-priced new-gas reserves.

The third test relates downstream margins to average wellhead prices. The positive relation that is graphed is said to indicate that old-gas rents are not totally dissipated by pipelines/distributors, either through complete passthrough to consumers or through payment of extraordinary new-gas prices to producers.

First, what is graphed is at best rent on a single input, not overall rents to the downstream. A recent Natural Gas Supply Association study shows no overall increase in pipeline rates of return, say from 1979 through 1981, after the second oil shock. It does show a marked drop in 1985, with partial wellhead decontrol, a fact which would be consistent with Bill's alternative hypothesis. However, an extraordinary writeoff by one company accounts for three-fourths of the drop, somewhat reducing the generality of that piece of data.

Second, the graphical data do not rule out extraordinary prices for new gas. All they show is that retail gas prices rose more absolutely than average wellhead prices. Might not other costs have risen too? Were new facilities built which were more costly on average than previous facilities? Certainly the energy component of operating costs would be expected to track wellhead prices. There could be several explanations other than rent retention, and the above mentioned rate-of-return data do not rule them out.

My comments are not meant to imply that Niskanen's alternative hypothesis is wrong or disproven, but rather that the particular tests chosen do not provide evidence that can be regarded as supportive.

Michael E. Canes
Vice-President and Chief Economist
American Petroleum Institute
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THE AUTHOR responds:

Michael Canes's letter misrepresents the relevance of the empirical evidence in my article to the specific questions that I addressed:

1. "Did regulation reduce the price of delivered natural gas to consumers?"

The evidence that the ratio of the price of oil to the price of delivered gas was roughly stable, despite a large increase in the real price of oil, indicates that the regulation of natural gas did not, except temporarily, reduce the price of delivered gas. Canes observes that this evidence, by itself, may also be consistent with average-cost pricing if pipelines bid up the price of new gas. I agree. But that addresses a different question to which the second type of evidence is relevant.

2. "Did regulation increase the price of new gas purchased by pipelines?"

My evidence is that the price of new gas purchased by the major pipelines was independent of the price of their old gas and was only weakly dependent on the amount of their old gas under contract. The evidence is roughly consistent with the conclusion of a prior study by the Department of Energy. Canes observes that the regulation of the price of gas may have had a stronger effect on the volume of new gas purchased. I agree. But that is a different question to which another type of evidence, which has not yet been examined, is relevant.

3. "Who captured the rents?"

The evidence indicates that the downstream margin (the sum of the distribution charges by the pipelines and the local distribution companies) increased sharply in parallel with the increase in the price of delivered gas. From that evidence, plus the two above types of evidence, I conclude that the pipelines and distribution companies appeared to capture most or all of the rents from the price ceilings on old gas. Canes observes that the reported rates of return of the pipelines were only weakly dependent on the price of gas. I agree. But that addresses a different question of how the pipelines used these rents. The evidence of both substantial increases in the downstream margin and only weak effects on rates of return suggests that the regulation of natural gas led to the worst possible combination of outcomes—no reduction in the price to consumers, a reduced supply of old gas, and increased investment and other costs by the pipelines and distribution companies. The rents were apparently dissipated by increased distribution costs, not by either lower prices to consumers or by higher prices to the producers of new gas.

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The available evidence, I contend, is fully consistent with my conclusions relative to the specific questions that I addressed. Some of Canes's observations are also correct, but they bear on issues that have not yet been examined.

William A. Niskanen
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The FTC and Cigarette Ads

TO THE EDITOR:

George Santayana once said, "Those who cannot remember the past are condemned to repeat it." In the midst of the current furor over proposals to ban all advertising and promotion of cigarette products, John E. Calfee's excellent and timely article, "The Ghost of Cigarette Advertising Past," may just jog enough memories to help us avoid repeating past errors.

Calfee's scholarly analysis makes an important contribution to our understanding of advertising regulation. This research, undertaken during his tenure at the Federal Trade Commission, also underscores the very real value that consumers receive from the work of the commission's Bureau of Economics. (See Bureau of Economics Working Paper No.134 for a fuller account of Calfee's research.) But for an analysis such as this one, the history of past regulatory mistakes would go unknown, their lessons unheeded.

Calfee's article paints a compelling picture of how the informational and competitive benefits of advertising serve consumers. From 1953 to 1954, apparently responding to "fear advertising," cigarette smoking fell 9 percent. Then the FTC stepped in to stop the ads. As Calfee reports, this FTC action was accompanied by a halt in the precipitous decline in smoking. When advertising of tar and nicotine claims heated up in the late 1950s, the resulting "tar derby" was associated with a spectacular 40 percent decrease in cigarette tar and nicotine levels. Again, FTC action—this time in the form of a voluntary ban on tar and nicotine ads—brought progress to a halt.

An analysis pieced together almost 30 years after the fact cannot capture all causes and effects of the regulation of cigarette advertising. However, one conclusion that clearly emerges from this article is that regulatory agencies, then as now, sometimes shoot first—and don't ask questions later! For policy makers who are uncertain as to whether no advertising or vigorously competitive advertising better serves consumers, Calfee's research is an indispensable reference.

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