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REGULATION was first published in July 1977 "because the extension of regulation is piecemeal, the sources and targets diverse, the language complex and often opaque, and the volume overwhelming."

REGULATION is devoted to analyzing the implications of government regulatory policy and the effects on our public and private endeavors.

For the Record

Assessing the Routes to Regulatory Reform

UNDER A COVER HEADLINED "THREE Routes to Regulatory Reform," *Regulation* (Vol. 22, No. 2) has done a real service by juxtaposing three reform proposals— from a political scientist, an economist, and a constitutional lawyer. Each argues that his discipline provides the right reform, but it turns out that we need the insights of all three.

Political scientist Matthew McCubbins argues that the problem is not that Congress delegates its lawmaking power to regulatory agencies, but that it sometimes delegates in the wrong way. For him, the solution is for voters to use the political process to complain to Congress. He shows that Congress can try to influence how agencies use delegated lawmaking power either by appropriately shaping the substantive and procedural terms of the delegation (which he calls "ex ante controls") or by credibly threatening to go after the agency with a meat cleaver should it produce rules that upset too many legislators (which he calls "ex post controls").

Professor McCubbins thinks ex ante controls work pretty well, but others are not so sure. (See, for example, David B. Spence's "Managing Delegation Ex ante: Using Law to Steer Administrative Agencies," in *Journal of Legal Studies* 28 [1999]: 413.) Based on the supposed efficacy of ex ante controls, Professor McCubbins concludes: "the president and Congress have many opportunities to structure the

agency's decisionmaking so that it is more responsive to their preferences." He concludes that delegation does not necessarily produce a principal-agent problem and so is "not equivalent to the abdication of Congress's lawmaking authority."

Professor McCubbins is wrong in concluding there is no principal-agent problem because he mistakes the agent for the principal. It is the public, not the members of Congress and the president, that is the principal. In the Constitution, "the people" delegate lawmaking authority to their agents: Congress and the president. To minimize principal-agent problems, the Constitution includes ex ante controls (bicameralism and presentment) and ex post controls (at least a majority of those elected lawmakers is on the hook at the next election for the rules imposed by government on their watch). It is then no surprise that Professor McCubbins' ersatz principals can satisfy their "preferences" by delegating, for they are able to escape the Constitution's controls yet retain some power over regulation. So, although Professor McCubbins is correct that Congress and the president do not abdicate power when they delegate, they do evade responsibility. That is why I called my book on delegation *Power Without Responsibility*.

Although Congress and the president are happy with delegation, the legitimate principals lose out, as shown in the economist's article (more on that later). Professor McCubbins's suggestion that the people can use the political process to get legislators to impose different controls on agencies overlooks the reality that voters have little purchase on legislators who have evaded the Constitu-

We welcome notes about current regulatory topics, letters that challenge or expand upon material we have published, and replies from authors. The writer's name, affiliation, address, and telephone number should be included. We cannot publish all the letters we receive, and we may reject any letter at our discretion. We may edit letters for length, clarity, and conformity to our editorial style.

tion's *ex post* controls on their own conduct. Because he has misidentified the principal, he is wrong as a matter of principle.

Economist Randall Lutter argues that the way to confine delegated law-making authority is to set up an independent federal agency to produce cost-benefit analyses of proposed regulations. Mr. Lutter points out that such second-guessing of agencies' own analyses is "a public good and thus is likely to be undersupplied by private markets."

The question, though, is whether this public good would do much good. Mr. Lutter says it will because it will "make regulatory decisionmakers more accountable to the public and Congress." Professor McCubbins, I think, would say that Mr. Lutter's proposal would have an impact both *ex ante* and *ex post*, and I would tend to agree. But I am not optimistic that the public would gain much, for a combination of reasons. The regulatory agency (not Mr. Lutter's watchdog agency) would still have authority to promulgate regulations and would be entitled to deference in court. Legislators could still disclaim responsibility for what the regulatory agency does. A report from Mr. Lutter's new agency claiming that the regulatory agency's cost-benefit analyses are way off would be unlikely to produce a scandal with which legislators must deal. EPA, for example, would have counterarguments about plausible differences in risk analyses, monetizing death and disease, discounting the future loss of human life, and more. Whatever the merits of the arguments, they would create more than enough noise to fuzz the debate. Long before such disputes were resolved, the public would tune out, more concerned with *who* pays the costs and *who* reaps the benefits than with the cost-benefit ratio.

Mr. Lutter's proposed agency would, however, make a big difference if regulations from EPA and other agencies had to be enacted by Congress

before they could go into effect. That arrangement would be required by the Congressional Responsibility Act bill, which Rep. J.D. Hayworth and Sen. Sam Brownback have introduced as one way for Congress to obey the Constitution's prohibition on delegation. Were their bill to become law, legislators would have not only the information from Mr. Lutter's agency but also the responsibility for the regulations that get imposed.

Constitutional lawyer Gary Lawson suggests enforcing the Constitution's prohibition on Congress's delegation of its lawmaking authority. Professor Lawson locates the constitutional prohibition in the "necessary and proper" clause. His analysis is sophisticated and plausible. He then

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addresses the question of how the courts should define unconstitutional delegation. He proposes that "Congress must make whatever policy decisions are sufficiently important to the statutory scheme at issue so that Congress must make them." My own proposal is that Congress must state the rules of conduct rather than authorizing agencies to do so. The purposes of our tests are the same: to make Congress accountable. But he gets my test wrong by taking one of my statements about its purpose for the test itself. I prefer my formalistic test to Professor Lawson's instrumental one because courts are less apt to defer to Congress when they have a formalistic test to apply. In any event, I agree with Professor Lawson that the differences between us are small.

In sum, trying to reform regulation without ending delegation may produce a lot of action, but it won't do much good. Making Congress take responsi-

bility the constitutional way would put both politics and economics in the service of real regulatory reform.

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More on the Failure of "Market Failure"

THE REAL ISSUE

PAUL BALLONOFF'S "ON THE FAILURE OF Market Failure" (*Regulation*, Vol. 22, No. 2) is an interesting and useful critique of the Lawrence Berkeley Laboratory (LBL) study of fluorescent-lighting ballasts.

However, Mr. Ballonoff's claim that "the case for market intervention rests on bad analysis" appears to be based on only one study, which evaluates the economic benefits of a single type of demand-side management (DSM).

Mr. Ballonoff finds the LBL study flawed and asserts that it "is the premier

example of market failure used by the energy conservation lobby to justify government intervention in energy markets." From that he concludes: "the case for [government] intervention is weak." He asks us to accept the proposition that a single study is representative of years of investigation and debate and, further, that the flaws he believes to have found in it invalidate all other serious studies of the topic.

Mr. Ballonoff's statement is remarkable for at least two reasons. First, although LBL certainly has done much work in the DSM area over the years, one study hardly qualifies, nor is acclaimed as "*the premier example of market failure*" (emphasis added). If it were, it would astound the thousands of engineers, economists, financial analysts, mathematicians, physicists, policy analysts, lawyers, policymakers, and others who have been grappling with market structure and DSM measurement and effectiveness issues since the mid-

1970s. There is a robust literature on DSM and the types of market failure that may prevent its wider adoption. For example, the proceedings of the nine International Energy Program Evaluation Conferences, held every two years since 1983, illustrate the scope, depth, rigor, and quality of DSM research as it has evolved since the 1970s and 1980s.

DSM was, in fact, adopted “by the market,” as evidenced by the success of energy service companies (ESCOs), which invested their own capital in energy-saving equipment and practices in return for a share of the savings on their clients’ utility bills. Clearly, DSM can and did produce real energy and dollar savings.

Of course, Mr. Ballonoff is not asserting that DSM per se is inappropriate or inefficient but, rather, that there is no credible evidence that government action to promote DSM is justified or produces net savings. That is an assertion and no more. There have been many studies measuring changes in energy consumption before and after application of a package of DSM measures. I, for example, participated in a study done for the National Bureau of Standards in 1977-78 that measured the thermal characteristics of various types of apartment buildings. That study was one of the first to show that heat loss through “non-obvious” infiltration pathways (e.g., up through uninsulated walls into and out of attics and attic crawl spaces) is much greater than heat loss through the more obvious pathways: windows and doors. Although the study did not measure the effectiveness of DSM measures, it was useful in identifying the types of measures that could be most effective. The issue of effective DSM evaluation and measurement continues to be intensely examined.

Clearly, there have been barriers to the use of otherwise economically efficient DSM. Those barriers have included improper regulatory pricing, as Mr. Ballonoff suggests. But there

are other barriers. For example, builders fail to construct highly efficient housing because of higher first costs and the nature of available financing; lenders lack information about the cost-effectiveness of various DSM measures; and low-income

The real issues surrounding demand-side management are not about market barriers or energy use but about the benefits: who reaps them and who should pay for them.

customers cannot afford to retrofit their dwellings to be more efficient, which would not only save them money but also reduce the bad-debt burden imposed on other utility customers through higher rates.

But the real issues surrounding DSM have not been about whether there are market barriers or if energy use declines after the application of DSM measures. Two key questions have been debated: What percentage of the change in consumption can be attributed to DSM measures? Is it appropriate to make nonparticipants pay for DSM measures that reduce total utility bills but perhaps lead to an increase in the price per unit of energy? In other words, what benefits flow from DSM, who reaps those benefits, and who should pay for them?

I believe that Mr. Ballonoff is concerned with who benefits and who pays. If that is so, he is re-entering the argument about efficiency versus equity. In essence, the argument is whether it is better to increase general economic welfare at the expense of a subset of the economy or to take only those actions that produce no losers, even if that means forgoing increases in the general welfare. Mr. Ballonoff appears to be in the “no losers” camp.

I submit that there is no objectively right answer to the argument. It ultimately requires a value judgment that turns on one’s personal philosophy. Moving to competitive markets and pro-

moting DSM have the same goal, that is, to increase economic efficiency. Competitive markets create losers as well as winners, while optimizing economic efficiency, if theory holds in practice. But there has been a debate over whether competitive markets have actually produced the efficiency gains claimed for them and who has received whatever gains have been produced. Is that debate conceptually different than arguments about the real value of DSM and whether the “no loser” standard should be applied? I submit that it is not.

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(The views and opinions expressed here are entirely the writer’s and do not necessarily represent those of the U.S. Department of Energy or its National Renewable Energy Laboratory.)

SETTING THE RECORD STRAIGHT

IN “ON THE FAILURE OF MARKET FAILURE” (*Regulation*, Vol. 22, No. 2), Paul Ballonoff makes many incorrect assertions about our case study of efficient magnetic ballasts, “Energy-Efficient Lighting: Market Data, Market Imperfections, and Policy Success,” written with Leslie J. Shown. We want to set the record straight. (Readers can find the full report in *Contemporary Economic Policy*, Vol. 14, No. 3 [July 1996], or as “Magnetic Fluorescent Ballasts: Market Data, Market Imperfections, and Policy Success” [LBNL-37702, December 1995], under 1995 reports at <http://enduse.lbl.gov/Info/Pubs.html>.)

Our research demonstrates beyond a reasonable doubt that the market does not always capture energy efficiency investment opportunities with high internal rates of return. Randall Lutter, writing on the importance of quality control in applied regulatory economics, states that “the gold standard in science is consistency with empirical evidence.” (See “The Role of Economic Analysis in Regulatory Reform,” in *Regulation*, Vol. 22, No. 2.)

Our paper presents such empirical evidence, but Mr. Ballonoff fails to grasp its origins or its import.

Mr. Ballonoff begins by making two mistakes in summarizing our analysis. First, he states that “the authors calculate the average cost of electricity in 1993 for three classes of commercial buildings that they describe very generally as having ‘high,’ ‘average,’ and ‘low’ use of electricity.” Our article makes it clear that we drew on a statistically representative sample of 5,000 commercial buildings, using electricity billing data and operating hours for individual buildings to avoid aggregation problems. We then sorted those results into the three categories that Mr. Ballonoff mentions (as well as into other categories), but the analysis was done at the level of individual buildings in the sample. Second, Mr. Ballonoff confuses the F40 and F96 designations for ballasts as measures of the efficiency or “advanced” nature of certain ballasts. In fact, they are merely names for the two main kinds of lamps, each of which is driven by a different type of ballast.

Mr. Ballonoff asserts that we made four errors in our analysis. We treat each of the four alleged errors in turn:

(1) His first criticism is that we used average- and not marginal-cost data for assessing the bill savings from the more efficient ballast. What is curious about this criticism is that we did not use cost data at all. In fact, we used *billing* data from each of our 5,000 representative buildings to find the average price of electricity for each building in the sample. Mr. Ballonoff’s excursion into the data on the variable costs of generating electric power is irrelevant and should simply be ignored. The only thing that matters for our argument is that the avoidable portion of a typical commercial sector electricity *bill* is virtually the entire amount of the bill. (The documentation can be found at <http://enduse.lbl.gov/shareddata/lettertoCEP.doc>.)

(2) Mr. Ballonoff asserts that falling electricity prices in the 1990s weaken our case. However, unless one believes that business firms are not merely rational but prescient, that trend is irrelevant to assessing the decisions made by commercial customers in the 1980s, which was the purpose of our analysis. The conventional approach, which we applied, is to approximate the expectations at the time decisions were undertaken. Our use of the Electricity Information Administration’s late-1980s forecast of roughly flat electrici-

There are many examples of institutional and market imperfections that lead to economic inefficiency. Some imperfections can be addressed by government policy.

ty prices was entirely appropriate for that purpose.

(3) Mr. Ballonoff then claims that we used optimistic estimates of ballast lifetimes. In fact, our ballast lifetimes are based on *empirical data* from the Lighting Research Institute, which surveyed ballast manufacturers, luminaire manufacturers, and lighting management companies. We referenced the data in our report. Mr. Ballonoff apparently pulls his estimate of 5 years out of the air; it has no empirical basis, to our knowledge.

(4) Finally, Mr. Ballonoff claims that the omission of 80 buildings with electricity prices above 20 cents per kilowatt-hour (kWh) from our sample significantly biases the results because those buildings “represent facilities with low use.” But because the 80 buildings represent only 1.2 percent of the floor area of our 5,000-building sample, the omission had little effect on our results. The 80 omitted buildings averaged 59 operating hours a week; the average for the entire sample was 79 hours a week. But the average electricity price for these same buildings was 22 cents per kWh,

or about *three times greater* than the average price for all buildings. Thus if we had included the 80 buildings in our sample, the higher price of electricity would have more than offset the effect of the lower operating hours, making our conclusions even stronger, albeit not by much.

In his “Policy Implications” section, Mr. Ballonoff embarks on a discussion about proper rate design, which is irrelevant to the argument given in our report. The translation of utility costs to rates is an interesting and important topic, but it has nothing to do with our analysis. We focused, correctly, on the bills that commercial customers in the 1980s actually faced, not bills as Mr. Ballonoff would have liked them to be designed.

Mr. Ballonoff’s critique thus fails on every count. He finds “peculiar” our conclusion that the ballast efficiency standards improved economic efficiency, but we are puzzled by his skepticism. The literature is rife with examples of institutional and market imperfections that lead to economic inefficiency. That some of these imperfections can be effectively and efficiently addressed by government policy should not be a surprising result. For a range of thoughtful opinions on the matter, we recommend the October 1994 issue of the journal *Energy Policy*.

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THE AUTHOR REPLIES

I WILL BEGIN WITH FOUR GENERAL comments about the preceding letters from Mr. Galen and Drs. Koomey and Sanstad. Then I will turn to methodological issues raised in the Koomey-Sanstad letter.

All three writers make the common mistake of confusing engineering efficiency with economic efficiency. The total energy conversion of a device is not the same as the economic efficien-

cy of that device, and engineering life is not the same as economic life.

As for empirical evidence, none of the writers can point to a specific study that shows that any government demand-side management (DSM) program ever produced benefits worth more than its costs. The massive literature they allude to promises massive benefits, but that is all. The literature may enumerate policy purposes that are political justification for government programs, but political agendas do not prove market failure.

It is certainly true, as Mr. Galen notes, that there are private companies that successfully sell energy efficiency services. But the existence of such companies hardly proves that government should create and impose DSM programs on unwilling consumers.

Contrary to Mr. Galen's suggestion, I am not merely concerned that there be "no losers" as a result of government programs. The issue is not who pays and who benefits. Again, the issue is the government's role in dictating to consumers and the lack of evidence that any government DSM program yields benefits greater than its costs.

I will turn now to specific methodological issues raised by Drs. Koomey and Sanstad.

First, they claim that I wrote about the cost of generating electricity, whereas their research was based on charges billed to customers (price per unit of electricity times the number of units). In fact, government regulators set utility tariffs—the prices offered by regulated utilities—to correspond generally with the cost of generating power. Thus the average "price" computed from a utility bill is not only a price, it is an approximation of the utility's average cost for the units of energy supplied to the customer during the billing period. At best, it is an approximation of the utility's marginal cost, because that is what utility regulators strive to reflect in tariffs. As I showed in my article, the use of marginal costs would reduce significantly the estimated returns on investments in government-mandated ballasts. As a consequence of their misunderstanding of my analysis,

they dismiss it as "irrelevant." But the proper pricing of electricity is critical to efficient resource allocation. It is a matter too important to be left to regulators.

Drs. Koomey and Sanstad next point out that they assumed flat rather than falling electricity prices because that was the expectation of the Electricity Information Administration at the time they did their research. In other words, government *cannot* predict the cost of electricity (among many other things) but it should nevertheless mandate the use of "energy efficient" lighting ballasts.

Drs. Koomey and Sanstad then rebuke me for illustrating how rates of return would be reduced if government-mandated ballasts fail to last as long as assumed in their report. Their letter claims that the ballast lifetimes they used were based on "empirical data...referenced...in our report." But the report merely cites an underlying report prepared by the Lighting

Research Institute, the details of which are not reproduced. All we know is what Drs. Koomey and Sanstad said in their report and say in their letter, namely, that the Lighting Research Institute's estimates are based on "surveys of ballast manufacturers, luminaire manufacturers, and lighting management companies." Their inclusion of manufacturers' data in those surveys suggests that the so-called empirical data about ballast lifetimes are simply engineering estimates.

All of this leads to a few final observations. It is time for the government to look at mass of studies of "market failure" it has funded and ask what they have produced other than unrealized benefits and proposals for more studies. It is time to turn off the government-funding spigot. What is worth doing in the area of energy conservation, private industry can do.

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