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Abstract

Although the allocative efficiency benefits of competition are a tenet of microeconomic theory, the relation between competition and technical efficiency is less well understood. Neoclassical models of profit-maximization subsume static cost-minimizing behavior regardless of market competitiveness, but agency models of managerial behavior suggest possible scope for competition to influence cost-reducing effort choices. This paper explores the empirical effects of competition on technical efficiency in the context of electricity industry restructuring. Restructuring programs adopted by many U.S. states made utilities residual claimants to cost savings and increased their exposure to competitive markets. We estimate the impact of these changes on annual generating plant-level input demand for non-fuel operating expenses, the number of employees and fuel use. We find that municipally-owned plants, whose owners were for the most part unaffected by restructuring, experienced the smallest efficiency gains over the past decade. Investor-owned utility plants in states that restructured their wholesale electricity markets had the largest reductions in nonfuel operating expenses and employment, while investor-owned plants in nonrestructuring states fell between these extremes. The analysis also highlights the substantive importance of treating the simultaneity of input and output decisions, which we do through an instrumental variables approach.

JEL Codes: L11, L43, L51, L94, D24
Keywords: Efficiency, Production, Competition, Electricity restructuring, Electric Generation, Regulation

Economists have long argued that competition generates important efficiency benefits for an economy. These generally focus on allocative efficiency; the implications of competition for technical efficiency are less clear. Neoclassical models of profit-maximization subsume static cost-minimizing behavior by all firms, regardless of market competitiveness.[1] Agency models, however, in recognizing the interplay of asymmetric information with the separation of management and control, suggest possible deviations from cost-minimization by effort-averse managers. These models may imply a role for competition in constraining managerial behavior, by rewarding efficiency gains and confronting less-efficient firms with the choice of cost reduction to the level of their lower-cost counterparts or exit; see Nickell (1996) for a brief discussion of some of these theoretical arguments. Their actual relevance is ultimately an empirical question.

This paper assesses the effect of competition on technical efficiency using data on the U.S. electric generation sector. The past decade has witnessed a dramatic transformation of this industry. Until the mid-1990s, over ninety percent of the electricity in the US was sold by vertically-integrated investor-owned utilities (IOUs), most operating as regulated monopolists within their service areas. Today, non-utility generators own roughly a quarter of generation capacity nationwide, and IOUs in many states own only a small fraction of total generating capacity and operate in a partially deregulated structure that relies heavily on market-based incentives or competition. While studies of state-level electricity restructuring suggest politicians may have been motivated in large part by rent-seeking (e.g., White, 1996, and Joskow, 1997), many proponents of restructuring argued that exposing utilities to competitive, market-based outcomes would yield efficiency gains that could ultimately reduce electricity costs and retail prices. Research on other industries suggests productivity gains associated with deregulation (e.g., Olley and Pakes, 1996, on telecommunications and Ng and Seabright, 2001, on airlines) and with increased competitive pressure caused by factors other than regulatory change (e.g., Galdón-Sánchez and Schmitz, 2002, on iron ore mines).[2]

[1] The implication of competition for dynamic efficiency through innovation is the subject of an extensive theoretical and empirical literature in economics, dating at least from Schumpeter's 1943 classic *Capitalism, Socialism, and Democracy*.

[2] Some hint of this possibility in electricity is provided by Primeaux (1977), who compared a sample of municipally owned firms facing competition to a matched sample of municipally owned firms in monopoly situations and found a significant decrease in costs per kWh for firms facing competition.

The considerable body of academic work on electricity restructuring within the U.S. and abroad has thus far focused on assessing the performance of competitive wholesale markets, with particular attention to the exercise of market power (see for example Borenstein, Bushnell and Wolak, 2002 and Joskow and Kahn, 2002). While many of the costs of electricity restructuring have been intensively studied, relatively little effort has been devoted to quantifying any ex post operating efficiency gains of restructuring, although a few studies (e.g., Knittel, 2002) have analyzed efficiency effects of various incentive regulations in this sector.[3] This study provides the first substantial analysis of early generation efficiency gains of electricity restructuring. As such, it contributes to the broad economic debate on the role of competition in the economy and is of direct policy relevance to states contemplating the future of their electricity restructuring programs.

The results of this work indicate that plant operators most affected by restructuring reduced labor and nonfuel expenses, holding output constant, by roughly 5% or more relative to other investor-owned utility (IOU) plants, and by 15-20% relative to government- and cooperatively-owned plants, which were largely unaffected by restructuring incentives. These may be interpreted as the medium-run efficiency gains that Joskow (1997, p. 214) posits "may be associated with improving the operating performance of the existing stock of generating facilities and increasing the productivity of labor operating these facilities." Our work also highlights the importance of treating the simultaneity of input and output choice. Failing to recognize that shocks to input productivity may induce firms to adjust targeted output leads to overstatement of estimated efficiency effects, in some cases by a factor of two or more. While endogeneity concerns have been long recognized in the productivity literature, ours is one of the first studies of electric generation to compensate for this. Finally, we explore the sensitivity of the estimated efficiency impact to the choice of control group to which restructured plants are compared, and discuss the issues involved in determining the appropriate counterfactual.

[3] One exception is Hiebert (2002), who uses stochastic frontier production functions to estimate generation plant efficiency over 1988-1997. One set of independent variables he includes is indicators for regulatory orders or legislative enactment of restructuring reforms in 1996 and in 1997. While he finds significant reductions in mean inefficiency associated with restructuring laws in 1996 for coal plants, he finds no effects for gas plants, nor for either fuel type in 1997. Our work uses a longer time period, richer characterization of the restructuring environment and dating of reforms consistent with the U.S. Energy Information Administration, and an alternative technology specification that allows for more complex productivity shocks and treats possible input endogeneity biases. Joskow (1997) describes the significant labor force reductions that accompanied restructuring in the UK, as the industry moved from state-owned monopoly to a privatized, competitive generation market, although these mix restructuring and privatization effects.

The remainder of the paper is organized as follows: Section 1 describes existing evidence on the competitive effects of efficiency, and discusses how restructuring might alter electric generation efficiency. Section 2 details our empirical methodology for testing these predictions, and describes our strategy for identifying restructuring effects. The data are described in Section 3. Section 4 reports the results of the empirical analysis, and Section 5 concludes.

1. Why Might Restructuring Affect Generator Efficiency?

Exit by less-efficient firms is a well-understood efficiency benefit of competition: as output shifts from (innately) higher-cost firms to lower-cost competitors the total production cost for a given output level decline. Olley and Pakes (1996) provide empirical evidence of this phenomenon in their plant-level analysis of the magnitude and source of productivity gains in the U.S. telecommunications equipment industry over 1974-1987. They find substantial increases in productivity associated with the increased competition that followed the 1984 divestiture and deregulation in this sector, and identify the primary source of these gains as the re-allocation of output from less productive to more productive plants across firms. In a similar vein, Syverson (2004) finds that more competitive local markets in the concrete industry are associated with higher mean, less dispersion, and higher lower-bounds in plant productivity, effects he attributes to the exit of less-efficient plants in more competitive environments.

The existing evidence on whether competition also leads to cost reductions through technical efficiency gains by continuing producers and plants is relatively sparse. Nickell (1996) uses a panel of 670 U.K. manufacturing firms to estimate production functions that include controls for the competitive environments in which firms operate. He finds some evidence of reduced productivity levels associated with market power and strong support for higher productivity growth rates in more competitive environments. Concerns about the ability of cross-industry analysis to control adequately for unobservable heterogeneity across sectors may make sector-specific evidence tighter and more convincing.[4] A notable example is the Galdón-Sánchez and Schmitz (2002) study of labor productivity gains at iron ore mines that faced increased competitive pressure following the collapse of world steel production in the early 1980s. They find unprecedented rates of labor productivity gains associated with this increase in competitive

[4] A number of studies have analyzed efficiency gains following regulatory reform in various industries; see, for example, Bailey's (1986) overview and Park et al. (1998) on airlines. Unfortunately, in many cases it is difficult to disentangle direct regulatory effects on efficiency (e.g., operating restrictions imposed on trucking firms or airlines by regulators in those sectors) from the indirect effects of reduced competition.

pressure, "driven by continuing mines, producing the same products and using the same technology as they had before the 1980s" (Galdón-Sánchez and Schmitz, 2002, p. 1233).[5]

Several features of the electric generation sector make it an attractive subject for testing potential competitive effects on technical efficiency.[6] First, generation technology is reasonably stable and well-understood and data on production inputs and outputs at the plant-level are readily available to researchers. This has made electric generation a common application for new production and cost function estimation techniques, dating at least to Nerlove (1963). Second, policy shifts over a relatively short period have resulted in a dramatic transformation of the market for electric power. Through the early 1990s, the U.S. electricity industry was dominated by vertically integrated investor-owned utilities (IOUs). Most operated as regulated monopolists over generation, transmission, and distribution of electricity within their localized geographic market, though there was some wholesale power traded among utilities or purchased from a small but growing number of non-utility generators. Prices generally were determined by state regulators based on accounting costs of service at the firm level. By 1998, every jurisdiction (50 states and the District of Columbia) had initiated formal hearings to consider restructuring their electricity sector, and by 2000, almost half had approved legislation introducing some form of competition including retail access.[7] This provides both time series and geographic variation in competitive environments. Third, static and dynamic efficiency claims bolstered much of the policy reform; measuring these benefits is a vital prerequisite to assessing the wisdom of these policies.

It has long been argued that traditional cost-of-service regulation does relatively well in limiting rents but less well in providing incentives for cost-minimizing production; see Laffont and Tirole (1993). Under pure cost-of-service regulation, regulator-approved costs of the utilities are passed directly through to customers, and reductions in the cost of service yield at most short-term profits until rates are revised to reflect the new lower costs at the next rate case.[8] Given asymmetric information between regulators and firms, inefficient behavior by managers that raises operations costs above minimum cost levels generally would be reflected in increased rates

[5] Ng and Seabright (2001) estimate cost functions for a panel of U.S. and European airlines over 1982-1995, and conclude that potential gains from further privatization and increased competition among European carriers are substantial, though they point out that the best-measured component of these gains relates to ownership rather than market structure differences.
[6] Understanding possible reallocation of output across plants is hampered by the exit of plants from most available databases when they are sold to non-utility owners.
[7] In the aftermath of California's electricity crisis in 2000-2001, restructuring has become less popular and many states have delayed or suspended restructuring activity, including six that had previously approved retail access legislation. See US Energy Information Administration (EIA), 2003.

and passed through to customers. Joskow (1974) and Hendricks (1975) demonstrate that frictions in cost-of-service regulation, particularly those arising from regulatory lag (time between price-resetting hearings), may provide some incentives at the margin for cost-reducing effort. Their impact generally is limited, however, apart from periods of rapid nominal cost inflation (see Joskow, 1974).

This system led economists to argue that replacing cost-of-service regulation with higher-powered regulatory incentive schemes or increased competition could enhance efficiency.[9] Over the 1980s and early 1990s, many state utility commissions accordingly adopted some form of incentive regulation. The limited empirical evidence available on these reforms, which modify price setting within the regulated monopoly structure, suggests mixed results. Knittel (2002) studies a variety of incentive regulations in use through 1996, and finds that those targeted at plant performance or fuel cost were associated with gains in plant-level generation efficiency.[10] More general reforms, such as price caps, rate freezes, and revenue-decoupling programs, typically were associated with insignificant or negative efficiency estimates, all else equal.

Restructuring, in contrast to incentive regulations, fundamentally changed the way plant owners earn revenue. At the wholesale level, plants sell either through newly created spot markets or through long-term contracts that are presumably based on expected spot prices. In the spot markets, plant owners submit bids indicating the prices at which they are willing to supply power from their plants. Dispatch order is set by the bids, and, in most markets, the bid of the marginal plant is paid to all plants that are dispatched. High-cost plants will be forced down in the dispatch order, reducing likely revenue. Plant operators that reduce costs move higher in the dispatch order, increasing dispatch probability, and increase the profit margin between own costs and the expected market price. Most restructuring programs also changed the way retail rates are determined and the way in which retail customers are allocated.[11] Retail access programs in

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[8] Rates are constant between rate cases, apart from certain specific automatic adjustments (such as fuel adjustment clauses), so changes in cost would not be reflected in rates until the next rate case.
[9] See, for example, Laffont and Tirole, 1993, for a theoretical justification, or Joskow and Schmalensee, 1987, for an applied argument.
[10] Knittel uses OLS and stochastic production frontier techniques to estimate Cobb-Douglas generating plant production functions in capital, labor, and fuel for a panel of large IOU plants over 1981-1996. His results from first-differenced models, which implicitly allow for plant-level fixed efficiency effects, suggest gains on the order of 1-2% associated with these reforms. Equations that do not allow for plant fixed effects suggest much larger magnitudes.
[11] States have used a variety of approaches to link retail rates under restructuring to wholesale prices in the market. Over the short term, most states decoupled utility revenue from costs by mandating retail rate freezes, often at levels discounted from pre-restructuring prices. Some states, such as Pennsylvania, are aggressively trying to encourage entry by competitive energy suppliers, who may contract directly with retail customers.

combination with the creation of the new wholesale spot markets may increase the intensity of cost-cutting incentives, leading to even greater effort to improve efficiency.

While the most significant savings from restructuring are likely to be associated with efficient long-run investments in new capacity, there may be opportunities for modest reductions in operating costs of existing plants (see Joskow, 1997). This paper attempts to measure the extent of that possible improvement for the existing stock of electricity generating plants in the U.S. The implicit null hypothesis is that, before restructuring, operators were minimizing their costs, given the capital stock available in the industry. Under the null, there should be no change in plant-level efficiency measures associated with restructuring activity. We discuss below our method for estimating plant efficiency and identifying deviations from this hypothesis. Assessing the effects of restructuring requires specification of how generating plants would have been operated absent the policy change. Constructing this counterfactual is crucial, but difficult.

2. Empirical Model

For a single-output production process, productive efficiency can be assessed by estimating whether a plant is maximizing output given its inputs and whether it is using the best mix of inputs given their relative prices. Production functions describe the technological process of transforming inputs to outputs and ignore the costs of the inputs; a plant is efficient if it is on the production frontier. Cost minimization assumes that, given the input costs, firms choose the mix of inputs that minimizes the costs of producing a given level of output. A plant could be producing the most output possible from a given input combination, but not minimizing costs if, for instance, labor was cheap relative to materials, yet the plant used a lot of materials relative to labor. Even if the firm were producing the maximum output possible from its workers and materials, it would not be efficient if it could produce the same level of output less expensively by substituting labor for materials. We explore the impact of restructuring on efficiency by specifying a production function and then deriving the relevant input demand equations implied by cost minimization.

We adopt the convention of representing generating plant output (Q) by the net energy the generating units produce over some period (measured by annual megawatt-hours, MWh, in our data), a choice that is discussed in further detail in the data section below. While a multitude of studies of electric plant productivity model this output as a function of current inputs, often using a Cobb-Douglas production or cost function, the characteristics of electricity production argue strongly for an alternative specification. We derive a model of production and cost minimization that is sensitive to important institutional characteristics of electricity production that have been largely ignored in the earlier literature.

First, observed output in general will be the lesser of the output the plant is capable of producing, given its available inputs, and the output called for by the system dispatcher. Because the system dispatcher must balance total production with demand at each moment, the gap between probable (Q^P) and actual (Q^A) output for a given plant *i* will be a function of demand realizations, the set of other plants available for dispatch, and plant *i*'s position in the dispatch order.[12]

Second, while fuel inputs are varied in response to real-time dispatching and operational changes, other inputs to a plant's production are determined in advance of output realizations. Capital typically is chosen at the time of a unit's construction (or retirement), and at the plant level is changed relatively infrequently. From the manager's perspective, it may be considered a fixed input. Utilities hire labor and set operating and materials expenditures in advance, based on expected demand. While these can be adjusted over the medium-run, staffing decisions as well as most maintenance expenditures are not tied to short-run fluctuations in output.[13] We therefore treat these as set in advance of actual production, and determining a target level of probable output, Q^P.

Finally, while labor, materials, and capital may be to some extent substitutable to produce probable output, the generation process generally does not allow these inputs to substitute for fuel in the short-run. Given this description of the technology, we posit a Leontief production process for plant *i* in year *t* of the following form:

$$Q_{it}^{A} = \min[\, g(E_{it}, \Gamma^{E}, \varepsilon_{it}^{E}),\ Q_{it}^{P}(K_i, L_{it}, M_{it}, \Gamma^{P}, \varepsilon_{it}^{P})\cdot\exp(\varepsilon_{it}^{A})]$$

where Q^A is actual output and Q^P is probable output; inputs are denoted by E for energy (fuel) input, K for capital, L for labor, and M for materials; Γ denotes parameter vectors, and ε denotes unobserved (to the econometrician) mean zero shocks. See Van Biesebroeck (2003) for the derivation of a similar production function he uses to model automobile assembly plant production.

[12] Random shocks to a plant's operations, such as unexpected equipment failures or equipment that lasts longer than expected, will cause it to produce less or more than its probable output from a set of available inputs.

As noted above, fuel input decisions are made in real time, after the manager has observed any shocks associated with the plant's probable output productivity, ε_{it}^{P}, the actual operation of the plant, ε_{it}^{A}, and the plant's energy-specific productivity in the current period, ε_{it}^{E} . Probable output, Q^{P}, is in contrast determined by input decisions made in advance of actual production. We assume that capital, measured by the nameplate generating capacity of the plant, is fixed.[14] Labor and materials decisions are made in advance of production, but after the level and productivity of the plant's capital is observed. This reflects the quasi-fixity of these inputs over time: staffing decisions and maintenance plans are designed to ensure that the plant is available when it is dispatched, based on the targeted output Q^{P}. The error term ε_{it}^{P} incorporates productivity shocks that we assume are known to the plant manager in advance of scheduling labor and materials inputs, but are not observable to the econometrician. We allow actual output to differ from probable output by a multiplicative shock $\exp(\varepsilon_{it}^{A})$, assumed to be observed at the time fuel input choices are made but not known at the time probable output is determined. This shock would be, for example, negative if a generating unit were unexpectedly shut down due to a mechanical failure, or positive if the plant were run more intensively than anticipated, as might be the case if a number of plants ahead of it in the usual dispatch order were unavailable or demand realizations were unexpectedly high.

We model probable output (Q^{P}) with a Cobb-Douglas function of labor and materials and embedding capital effects in a constant ($Q_0(K)$) term. This yields the specification:

(PF1) $$Q_{it}^{P} \leq Q_0(K_i)\cdot(L_{it})^{\gamma L}\cdot(M_{it})^{\gamma M}\cdot \exp(\varepsilon_{it}^{P})$$

In preliminary analysis, we estimated the parameters of the production function, including terms that allowed for differential productivity under restructuring. Those results suggested productivity gains associated with restructuring. The work reported here imposes an additional constraint, based on cost-minimization, to estimate input demand functions, and isolate possible restructuring effects on each measured input. A cost-minimizing plant manager, facing wages W_{it} and material prices S_{it}, would solve for the optimal inputs to produce probable output Q_{it}^{P} by:

[13] In fact, over a short time period, maintenance and repair expenditures will be inversely related to output since the boiler needs to be cool and the plant offline for most major work. We deal with this potential simultaneity bias below.

[14] The empirical analysis defines a new plant-epoch, *i*, whenever there are significant changes in capacity, so that within each plant-epoch, capacity is approximately constant.

$$\min_{L_{it},\, M_{it}} \quad W_{it}\cdot L_{it} + S_{it}\cdot M_{it} \qquad \text{s.t. } \underline{Q}_{it}^{P} \le Q_0(K_i)\cdot(L_{it})^{\gamma L}\cdot(M_{it})^{\gamma M}\cdot\exp(\varepsilon_{it}^{P})$$

yielding the following factor demand equations:

(L1) $L_{it} = (\lambda\gamma^L Q_{it}^P)/W_{it}$

(M1) $M_{it} = (\lambda\gamma^M Q_{it}^P)/S_{it}$

where λ is the Lagrangian on the production constraint.

We observe actual output, $Q_{it}^A = Q_{it}^P\varepsilon_{it}^A$, rather than probable output, Q_{it}^P. Making this substitution and taking logs of both sides, (L1) becomes:

(L2) $\ln(L_{it}) = \alpha_0 + \ln(Q_{it}^A) - \varepsilon_{it}^A - \ln(W_{it})$

where $\alpha_0 = \ln(\lambda\gamma^L)$. If there are differences across plants, over time, or across regulatory regimes in the coefficients of the production function (γ^L) or in the shadow value of the probable output constraint (λ), or if there is measurement error in labor used at the plant, this equation will hold with error. As we are particularly interested in changes in input demand associated with restructuring, we expand the subscript *it* to *irt* to include plant *i* in year *t*, and regulatory restructuring regime *r*, and re-write (L2) as: [15]

(L2') $\ln(L_{irt}) = \ln(Q_{irt}^A) - \ln(W_{irt}) + \alpha_i^L + \delta_t^L + \varphi_r^L - \varepsilon_{irt}^A + \varepsilon_{irt}^L$

where α_i^L measures a plant-specific component of labor demand, δ_t^L captures year-specific differences in labor demand, φ_r^L captures restructuring-specific shifts in labor demand, and ε_{irt}^L measures the remaining error in the labor input equation. α_0 is now subsumed in the plant-specific demand, α_i^L. Note that φ_r^L picks up mean residual changes in labor input for a plant in a restructured regime relative to that plant overall and to all other plants at the same point in time. It could reflect systematic changes in the marginal productivity of labor (γ^L), in the shadow value of the availability constraint (λ) or in optimization errors. [16]

[15] Note that many plant-level differences, such as capital stock, and many time-varying shocks, such as technology-neutral productivity shocks, drop out of this equation through the conditioning on output choice.

[16] If there were systematic differences in the relation of probable and actual output across restructuring, γ_r^L may also reflect the change in mean ε_{irt}^A. Since ε_{irt}^A reflects shocks unobservable by the firm when setting planned output, it seems plausible that these be mean zero in expectation, but their realizations could be nonzero in the restructuring sample we observe.

Similarly, equation (M1) becomes:

(M2) $$\ln(M_{irt}) = \ln(Q_{rt}^{A}) - \ln(S_{rt}) + \alpha_i^{M} + \delta_t^{M} + \varphi_r^{M} - \varepsilon_{irt}^{A} + \varepsilon_{irt}^{M}$$

which is directly analogous to (L2').

We model the energy component of the Leontief production function, which will in general hold with equality, as:

(PF2) $$Q_{irt}^{A} = g(E_{irt}, \gamma^{E}, \varepsilon^{E})$$

Assuming that g(•) is monotonically increasing in E, we can simply invert it to get an expression for E in terms of Q. Note that the price of fuel does not enter into the demand for fuel except through the level of output the plant is dispatched to produce. For consistency with the other input specifications, we specify a log-log relationship:

(E1) $$\ln(E_{irt}) = \gamma_Q^{E} \cdot \ln(Q_{irt}) + \varphi_r^{E} + \alpha_i^{E} + \delta_t^{E} + \varepsilon_{irt}^{E}$$

where as before, the plant-specific error, α_i^{E}, the year-specific error, δ_t^{E}, and the restructuring-specific term, φ_r^{E}, capture systematic changes in the efficiency with which plants convert energy to electricity—that is, changes in plant heat rates—across plants, over time, or correlated with restructuring activity, respectively.

We confront two important endogeneity concerns in estimating the basic input demand equations, (L2'), (M2) and (E1). The first is the possibility that shocks (ε_{irt}^{L}, ε_{irt}^{M}, ε_{irt}^{E}) in the input demand equations may be correlated with output. If output decisions are made after a plant's manager observes the plant's efficiency, managers may increase planned output in response to positive shocks to an input's productivity, or reduce planned output in response to negative shocks. This behavior would induce a correlation between the error in the input demand equation and observed output. Though one can control directly for plant-specific efficiency differences and for secular productivity shocks in a given year, idiosyncratic shocks remain a source of possible bias. Second, the estimates may be subject to selection bias if exit decisions are driven by unobserved productivity shocks. In this case, negative shocks could lead to plant shutdown, implying that the errors for observations we observe will be drawn from a truncated distribution. Neither of these problems is unique to our setting, and they have been raised in many earlier papers.[17]

Consider first the simultaneity issue. We face a potential simultaneity problem if, for instance, a malfunctioning piece of equipment reduces the plant's fuel efficiency, leading the utility to reduce its operation of that plant and consequently to use less fuel. There may be deviations from predetermined employment and materials budgets caused by unanticipated breakdowns that require increased use of labor and repair expenditures and result in lower output. A positive efficiency shock to an input may lead managers to run that plant more intensively over the year, increasing output as well as input use. A variety of methods have been used to address this concern.[18] We choose to use an instrumental variables approach, using a measure of state-level electricity demand as an instrument for plant output. This is likely to be highly correlated with the amount of output a plant will be called to provide, but uncorrelated, for instance, with how efficiently an individual plant's feedwater pumps are working. This approach is likely to be particularly effective for the energy equation, given the responsiveness of energy input choices to demand fluctuations in real time, and for identifying exogenous output fluctuations at non-baseload plants, which are more strongly influenced by marginal swings in demand. It may be less powerful in identifying variation in ex ante labor and maintenance choices, depending in part on the extent to which plant managers anticipate state demand.[19] We therefore explore the sensitivity of our results to alternative instruments.

The potential selection issue is more difficult to address. The plants in our sample seem more stable than those studied in many other contexts (especially see Olley and Pakes, 1996), suggesting that the selection problem may be somewhat less severe for electric generation. However, plant exit increases in restructuring regimes, typically not because the plant is retired but because divestitures remove the plants from the reporting database. To the extent that the divestitures are mandated by the restructuring legislation, this should not create selection problems. But without better information on what determines discretionary divestitures, we have

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[17] Nerlove (1963) provides an early discussion of simultaneity bias in production functions. Olley and Pakes (1996) propose a structural approach to addressing simultaneity, which is compared to alternatives in Griliches and Mairesse (1998). Ackerberg and Caves (2003) discuss this issue and compare treatments proposed by Olley and Pakes (1996) and Levinsohn and Petrin (2003). While many papers have estimated production or cost functions for electric generating plants, from the classic analyses in Nerlove (1963) and Christensen and Greene (1976) to very recent work such as Kleit and Terrell (2001) and Knittel (2002), electricity industry studies typically have not treated either simultaneity or selection problems.

[18] See the references cited in note 17, supra.

[19] A further drawback to this instrument is that we measure demand only at the state, rather than plant level, which means that we do not use the cross-plant variation within a state to identify output coefficients.

no direct way to assess their impact on the results. One indirect way to assess the significance of potential selection effects is to compare results for the unbalanced panel we use in most of our work to those for a panel of plants that continue to operate through the end of our sample period, for which potential selection effects are likely to be most severe. Substantial differences across those results may suggest the need to more carefully treat potential selection biases.

Identification strategy

There is substantial heterogeneity across plants, utilities and states, and the economic environment in which utilities operate has changed considerably over time. In addition, restructuring is not randomly assigned across political jurisdictions—earlier work suggests that it is strongly correlated with higher than average electricity prices in the cross-section.[20] Fortunately, we have in this sector a database rich in variation. There are thousands of generating plants operated by hundreds of utilities subject to regulation by dozens of political jurisdictions each setting their own legal and institutional environment. Panel data on the costs and operations of these plants are available, with some recent exceptions, from well before any restructuring until the present.[21] This allows us to construct benchmarks that we believe control for most of the potentially confounding variation.

The plant-specific effects, $\{\alpha_i^N\}$, measure the mean use of input N at plant i relative to other plants in the sample. These effects may be associated with differences in plant technology type and vintage, ownership (government v. private utilities), and time-invariant state effects. The year-specific shock, $\{\delta_t^N\}$, measures the efficiency impact of sector-level shifts over time, such as secular technology trends, macroeconomic fluctuations or energy price shocks. Restructuring effects on plant productivity correspond to a non-zero $\{\varphi_r^N\}$. The heterogeneity in the timing and outcomes of state-level restructuring activity allow the data to distinguish between temporal shocks and restructuring effects. While all states held hearings on possible restructuring, the earliest was initiated in 1993 and the latest in 1998. There is considerable variation in the outcome of those hearings, as well, with just under half the jurisdictions (23 states and the District of Columbia) enacting restructuring legislation between 1996 and 2000.[22] The remainder

[20] The significant role of sunk capital costs in regulatory ratemaking means that high prices do not necessarily imply high operating costs for generation facilities within a state, however. See Joskow (1997) for a discussion of the contributors to price variation across states.
[21] Cost data are not publicly available for plants owned by exempt wholesale generators, including those acquired from regulated utilities.
[22] We collected information on state restructuring legislation from various Energy Information Administration and National Association of Regulatory Utility Commissioners publications and state

considered and rejected, or considered and simply did not act on, such legislation. This variation allows us to use changes in efficiency at plants in states that did not pass restructuring legislation to identify restructuring separately from secular changes in efficiency of generation plants over time.

It is possible that plants in this control group also altered their behavior over the post-1992 period. This could be due perhaps to the introduction or intensification of incentive regulation within states that did not enact restructuring, to the expectation of potential restructuring that did not occur, or spillovers from restructuring movements in other states (e.g. if regulators updated their information about the costs necessary to run plants of a certain type, or multi-state utilities operating under differing regimes improved efficiency of all their plants, not just those in restructuring states). To the extent this occurs, our comparison will understate the magnitude of any efficiency effect of restructuring.

We therefore consider a second control group, consisting of cooperatively-owned or publicly-owned municipal and federal plants, which for convenience we will refer to as "*MUNI*" plants, although the group is broader than strictly implied by this label. An extensive literature has debated the relative efficiencies of private and public ownership in this sector under traditional regulation, with somewhat mixed results. We abstract from that by allowing for plant-specific effects that absorb any levels differences in input use across ownership type. Restructuring generally altered the competitive environment only for private investor-owned utilities within a state, leaving those for publicly- and cooperatively-owned utilities unchanged.[23] This suggests that *MUNIs* may provide a second benchmark against which to measure changes in efficiency associated with restructuring. We adopt a parameterization that measures $\{\varphi_r^N\}$ relative to publicly-owned plants during the period that investor-owned utilities are at risk of restructuring, defined as 1993 forward. Using *N* to denote input (labor, nonfuel expenses, or fuel), and $PRICE^N$ to denote the relevant input price (none for the fuel equation), we have input use equation I1:

$$\text{(I1)} \qquad \ln(N_{irt}) = \ln(Q_{irt}^{A}) - \ln(PRICE^{N}_{irt}) + \gamma MUNI_{it} + \alpha_i^N + \delta_t^N + \varphi_{ir}^{N} - \varepsilon_{irt}^{A} + \varepsilon_{irt}^{N}$$

public utility commission websites. Since 2000, no additional states have enacted restructuring legislation, and several have delayed or suspended restructuring activity in response to the California crisis.

[23] With the exception of Arizona and Arkansas, which included government-owned utilities in restructuring programs.

This specification implicitly provides two "non-treatment groups" to which investor-owned plants in restructuring regimes may be compared: investor-owned plants in non-restructuring regimes (with the restructuring effect measured by $\varphi_r^{\,N}$), and public- and cooperatively-owned plants over 1993-1999 (with the restructuring effect measured by $\varphi_r^{\,N} - \gamma$).

3. Data & Summary Statistics

The analysis in this paper is based on annual generating plant-level data for U.S. electric utilities. Plants are comprised of at least one, but typically several, generating units, which may be added to or retired from service over the several-decade life of a typical generating plant. While an ideal data set would allow us to explore efficiency at the generating unit level, inputs other than fuel are not available at the generating unit level, and some, such as employees, are not even assigned to the unit level as they are shared across units at the plant.[24] We therefore use a plant-year as an observation.

The Federal Energy Regulatory Commission (FERC) collects data for investor-owned utility plants annually in the FERC Form 1, and the Energy Information Administration (EIA) and Rural Utilities Service (RUS) collect similar data for municipally-owned plants and rural electric cooperatives, respectively. These data include operating statistics such as size of the plant, fuel usage, percentage ownership held by the operator and other owners, number of employees, capacity factor, operating expense, year built, and many other plant-level statistics. Our base data set includes all large steam and combined cycle gas turbine (CCGT) generating plants for which data were reported to FERC or EIA over the 1981 through 1999 period.[25] We excluded smaller plants, defined as those for which gross capacity exceeded 100 megawatts for fewer than three of our sample years. We also excluded approximately 1,500 observations where data were missing, and dropped several hundred observations based on regression diagnostic tests to screen for outliers or undue influence. Further details on the data are provided in the Appendix.

We follow the literature in characterizing output by the total energy output of the plant over the year, measured by annual net megawatt-hours of electricity generation, *NET MWhs*. This is an imperfect choice. Output is, in reality, multidimensional, although most dimensions are not

[24] Some labor may be shared across multiple plants, though assigned to one particular plant in our data. This will lead induce measurement error, particularly in the plant employment variable.
[25] One unfortunate consequence of restructuring is that available data on plants sold by utilities to non-utility generators are extremely limited after the sale, due to changed reporting requirements. This means that plants will be excluded from the dataset after such sales.

recorded in the plant data. For example, generating plants may also provide reliability services (such as spinning reserves, when the plant stands ready to increase output at short notice), voltage support and frequency control. While the production process varies considerably across these different outputs, only net generation is well measured in the data.[26] Moreover, electricity output is not a homogenous product. Because electricity is non-storable, electricity produced at 5PM on the first Friday in July is a separate output from electricity produced at 5AM on the second Sunday in March. Firms must decide how to balance the costs associated with taking their plant down to do maintenance against the probability that a poorly maintained plant will fail during peak demand hours, and the availability of the plant may be an important modifier of output quality. Changes in incentives associated with restructuring may have altered firms' assessments of these tradeoffs, although the expected direction of the effects is theoretically ambiguous.[27] Hourly output prices and output from individual plants might allow us to better assess this. Lacking such data, we rely on a single output dimension, but acknowledge its limitations.

We have information on three variable inputs. The first, *EMPLOYEES,* is a count of full-time employees at the plant. The second, *NONFUEL EXPENSE,* includes all non-fuel operations and maintenance expenses, such as expenses for coolants, maintenance supervision and engineering expenses. This variable is less than ideal as a measure of materials, both because it reflects expenditures rather than quantities, and because it includes the wage bill for the employees counted in *EMPLOYEES*, although that expense is not separately delineated in our data. As *NONFUEL EXPENSES* includes payroll costs (not separately identified), both this and *EMPLOYEES* will reflect changes in staffing.[28] The third input is fuel use by type of fuel (tons of coal, barrels of oil, and mcf of natural gas). We convert fuel into *BTU*s using the reported annual plant-specific Btu content of each fuel to obtain total *BTU* input at the plant for each year.

Input prices pose a challenge. Wages in particular may be endogenous to the firm and its perceived regulatory environment. Hendricks (1975) suggests that utilities may bargain less

[26] The inputs required to produce a given level of energy (MWh) from a specific plant also will depend on whether the plant runs continuously or intermittently and on its average capacity utilization. Starting a plant frequently and running it at low capacity utilization rates typically use more inputs (particularly fuel) per mwh generated than does running a plant continuously at its rated capacity.

[27] For instance, under traditional regulation, utilities may have faced strong political incentives to avoid blackouts or brownouts, leading to investment in greater capacity to increase reserve margins and in greater maintenance resources to increase plant reliability. On the other hand, competitive firms producing in restructured wholesale markets may face even stronger incentives to be available when demand peaks because this is when prices are highest.

[28] The elasticity of *NONFUEL EXPENSES* with respect to *EMPLOYEES* is about .5 in our data, broadly consistent with our back of the envelope calculations suggesting that labor costs are roughly half of the total nonfuel operating budget.

aggressively over input prices such as wages during periods in which higher costs could be readily passed on to customers through higher regulated prices, and more aggressively when the firm was likely to be the residual claimant to cost savings. In other industries, regulatory reform has sometimes been associated with substantial reductions in wages, suggesting rent-sharing under regulation (see Rose, 1987, on the trucking industry). These suggest that observed wages may not be exogenous to the firm, and may not reflect the opportunity cost to managers of the marginal unit of labor. We address this by using state-level average wages from industries with workers of similar skills and training to power plant operators, including natural gas distribution, petroleum refining and hazardous waste treatment facilities, denoted as *WAGE*. This reflects opportunity wages, and avoids confounding the employee price measurement with any changes in recorded wages due to changes in labor force composition at utilities associated with restructuring or changes in wage bargaining. We do not have plant- or even firm-specific indices for the materials prices that comprise *NONFUEL EXPENSES*. Our empirical model of *NONFUEL EXPENSES* therefore corresponds to an input demand equation with constant prices and a price coefficient of one.

The final input is the capital stock of the plant, which we measure by plant capacity and vintage. Our data record the plant capacity in megawatts. We combine this with information on unit retirements to define plant-epochs. Each plant is assigned a unique identifier. Any time the capacity of the plant is significantly changed, or there is an identifiable unit addition or unit retirement, we create a new identifier and associated new plant-specific effect. This allows capital changes to alter the underlying input efficiency of the plant.

We include controls for two other plant characteristics that may vary within plant-epoch and lead to changes in input use. The first is plant *AGE* in years, dated from the installation date of the oldest operating generating unit at the plant: as plants age they may become less efficient or require additional inputs for a given level of output. The second is the addition of a flue-gas desulfurization system, or *FGD* (also called scrubber), to reduce sulfur-dioxide emissions in some coal plants. *FGD* affects the environmental output, unmeasured by ln(*NET MWhs*).

We supplement the operational plant data with information on state-level restructuring activity. For each state, we have identified the date at which formal hearings on restructuring began, the enactment date for legislation restructuring the state’s utility sector, if any, the implementation date for retail access under that legislation, and some associated aspects of restructuring such as rate freezes and mandatory divestiture of generation. Testing for restructuring-specific shocks requires a determination of how to match this information with firm decisions: when were plant operators in a given state likely to have begun responding to a policy change? Consultations with industry participants and readings of these events suggest that utilities often acted in advance of final outcomes. The legislative and regulatory process leading up to state restructuring typically lasted a number of years, allowing utilities to anticipate the coming change, and alter their behavior in advance. For example, Boston Edison's 10-K filed in March 1994 discussed Massachusetts' consideration of restructuring, stating "The Company is responding to the current and anticipated competitive pressure with a commitment to cost control and increased operating efficiency without sacrificing quality of service or profitability" (p. 6).[29] Utilities may have begun to phase in input changes, especially those involving labor and particularly unionized workers. Moreover, as policy changes were discussed, rates were frozen in many states, either explicitly by policy makers or in effect by implicit PUC decisions not to hear new rate cases, enabling utilities to capture the savings from incremental cost reductions.[30]

In this work, we allow restructuring effects to begin with the opening of formal hearings on restructuring. The primary variable of interest, *RESTRUCTURED*, is an indicator variable that turns on with the start of formal proceedings in a state that eventually passed restructuring legislation.[31] A second variable, *RETAIL ACCESS*, indicates the start of retail access for plants in the four states that implemented retail competition during the sample.[32] Table 1 reports the number of plants in our database each year that were in states that had *RESTRUCTURED* and the

[29] In a 1993 article outlining PECO's cost saving accomplishments and strategies for the future, Chairman and CEO Joseph Paquette discussed restructuring of the utility industry and was quoted as stating, "we have been focusing on our strategic plans to enhance our abilities to satisfy our customer needs by becoming more competitive." PECO initiatives cited in the article included improving the cost effectiveness of all operations. One particular accomplishment noted was the reduction in total employment from 18,700 to 12,900.

[30] As noted earlier, some of these changes may have also affected utilities in non-restructuring states. For example, the number of utility rate cases dropped dramatically in the 1990s, implying that many or most utilities may have been short- or medium-run residual claimants to cost reductions. Knittel (2002) identifies a number of incentive regulations adopted in various jurisdictions during the 1990s. Many of the fuel-related regulations (modified pass-through clauses, heat rate and equivalent availability factor incentive programs) were strongly correlated with ultimate restructuring. Some of the broader regulations (e.g., price caps and revenue decoupling programs) were almost orthogonal to eventual restructuring.

[31] The *RESTRUCTURED* variable is based on whether a state had passed legislation as of mid-2001, although in the aftermath of the California electricity crisis, there has been no additional restructuring, and some delays or suspension of planned restructuring activity.

[32] While *RESTRUCTURED* indicates approval of retail access legislation, the specified phase-in of retail access was often slow. Only five states implemented retail access during our sample period: Rhode Island in 1997, California, Massachusetts, and New York in 1998, and Pennsylvania in 1999 (U.S. EIA, 2003). Because we have no valid observations on Rhode Island plants in 1997 or beyond, retail access effects will be determined by the 4 states implementing in 1998 or 1999. Divestiture requirements in California and Massachusetts further reduces the post-retail access sample of plants, as investor-owned plants in those states were largely divested by 1999.

number of plants in states that had started retail access by 1999.[33] If utilities did not respond until restructuring legislation or regulation was enacted and the policy uncertainty resolved, *RESTRUCTURED* will underestimate the true effect by averaging in non-response years. To evaluate this possibility we introduce a third variable, *LAW PASSED*, an indicator equal to one beginning in the year the state passes restructuring legislation.[34] Similarly if actual implementation of retail access and the associated wholesale market reforms is important to efficiency gains, it will be reflected in an incremental effect of *RETAIL ACCESS*. To examine municipally-owned plants over the restructuring time period, we define the variable *MUNI*POST 1992*, equal to one for all municipally-owned plants from 1993, the first year for which *RESTRUCTURED* is one, through 1999. The final column reports the number of plants in this category.

Details on the data sources and summary statistics are provided in the appendix. Tables 2a and 2b report summary statistics for plant-level data in 1985 across three categories: investor-owned plants in states that later restructure, investor-owned plants in states that do not restructure, and non-IOU plants. We choose this date to ensure that comparisons are made prior to any significant changes across states in the competitive or regulatory environment, even prior to restructuring initiatives. From these tables, it appears that the plants from these groups are not random draws from the same population. The first three variables measure employees and non-fuel operating expenses, scaled by the plant's capacity, and fuel use in millions of British thermal units (mmBtus), scaled by the plant's output. In 1985, before state-level restructuring initiatives were considered, plants in states that eventually restructured had higher intensities of employees and non-fuel operating expenses, although the difference is significant only for non-fuel expenses. The first two rows in Table 2b show that municipally-owned plants had significantly higher employment and non-fuel input use than plants in non-restructuring states. The differences in heat rates and capacity factors are not significant in either of the tables. The last four variables in both tables describe the stock of plants in the two types of states. Although plants are very similar in size across IOUs, MUNIs plants are considerably smaller. IOU plants in restructuring

[33] For the tables and the regression analysis that follows, plants are assigned to the state in which they are regulated. A plant located in one state may be owned by a company with exclusive service territory in a different state, and that second state is the state by which the regulatory policy is measured. Some plants are owned by a company with service territory in more than one state and some plants are owned by several companies that are regulated by different states. In the regression analysis, we found that separately characterizing "mixed" regulation and "shared" plants had very little impact on our results.
[34] There is on average about a 2.6-year lag between the initiation of hearings and the passage of the law. We have experimented with a number of alternative measures of restructuring activity, including variables that begin with hearings regardless of restructuring outcomes, those that measure years since hearings were

states tended to be older, more likely to use gas, and less likely to use coal, than IOU plants in non-restructuring states. MUNI plants tended to be younger, less likely to use coal, and more likely to use gas, than IOU plants in non-restructuring states. The regression analysis will control for these differences directly or with the use of plant-epoch effects.

If investor-owned utilities achieved efficiency improvements when facing impending restructuring of the generation sector, one would expect to see a relative decrease in the cost of generation for affected companies, and little difference in the change in transmission and distribution costs between the affected and not affected states since restructuring programs leave transmission and distribution comparatively untouched. If restructuring did not affect operating efficiency in the generation sector, we might expect either (1) the change in generation expenses would not be statistically different between restructuring and non-restructuring companies, or (2) we would see the same pattern of change in costs for the transmission and distribution sectors as for the generation sector.[35]

Table 3a and 3b display the difference in mean tests for investor-owned utilities in restructuring and non-restructuring states for a change in costs between 1990 and 1996. Table 3a reports the percentage change in total costs for each category of cost, and Table 3b reports the percentage change in costs per MWh. The larger decrease in generation costs at restructuring companies is significant at the 1% level, and the result for generation costs per MWh is significant at the 6% level. The difference in costs for companies in restructuring and non-restructuring states is not significant for either the transmission or distribution costs. These aggregate statistics provide preliminary support for the expectation that the portion of the utility company faced with competition (the generating sector) responded with a decrease in costs, while other sectors and companies not faced with competition did not share this response.

4. The Effects of Restructuring on Input Use

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initiated for states that eventually restructured, and the presence of restructuring-associated rate freezes. None of these seem to change materially to the conclusions we draw below.

[35] For the analysis comparing costs of generation, transmission, and distribution services, we rely on data reported annually by utility companies to the Federal Energy Regulatory Commission (FERC) in the FERC Form 1, page 320, 321, and 322 respectively. We use a balanced sample composed of all companies with data reported for all three sectors in both 1990 and 1996. This amounts to 49 companies in states that did not deregulate and 74 in states that did deregulate for the comparison of costs, and 48 and 72 respectively for the comparison of costs per MWh. Using costs per MWh necessitates the exclusion of a few companies for which MWh data was not available in one of the two years.

Following equation (I1), we estimate the influence of restructuring on the use of input *N* (*EMPLOYEES, NONFUEL EXPENSE,* and *BTUs*) with the following basic regression model:

$$\text{(R1)} \quad \ln(N_{irt}) = \beta_1^N \ln(NET\ MWh_{irt}) + \beta_2^N \ln(PRICE^N_{rt}) + \beta_3^k AGE_{irt} + \beta_4^k FGD_{irt} + \varphi_r^N IOU{*}RESTRUCTURED + \gamma MUNI_{1993\text{-}1999} + \alpha_i^N + \delta_t^N + \beta_1^N \varepsilon_{irt}^A + \varepsilon_{irt}^N$$

where we allow for non-unity coefficients on the output term (β_1^N) for all equations and on the input price term (β_2^N on *WAGE*) in the *EMPLOYEES* equation,[36] and include controls for two important plant characteristics that vary over time: *AGE* and *FGD* (scrubber). α_i^N is a time-invariant fixed effect for input *N* at plant-epoch *i*, which may contain a state-specific and ownership-specific error that will not be separately identified. These plant-specific effects control for much of the expected variation in input use across plants arising from heterogeneous technologies, state or regional fixed factors, and basic efficiency differences. They also control for differences in the plant mix between restructuring and non-restructuring states by comparing each plant to itself over time, removing any time-invariant plant effects. As a Hausman test rejects the exogeneity of plant effects, all reported results include plant-epoch fixed-effects.[37] δ_t^N is an industry-level effect in year t, which controls for systematic changes in input demand across all plants over time.

ε_{irt}^N is assumed to be a time varying mean zero shock for input *N* at plant-epoch *i* in regime *r* at time *t*. This shock is unlikely to be independent over time for a given plant. There is likely to be persistence in input shocks, particularly for labor, from year to year. Indeed, estimated rhos based on assumed first-order serial correlation are in the .65 range for labor inputs and in the .33 range for non-fuel expenses. It is unlikely that the correlation is as simple as a first-order autogressive process, however. The physical operation of power plants is likely to induce some correlation at longer differences. For example, routine maintenance cycles may involve scheduled shutdowns but increased labor and nonfuel expenses every three or four years. We have explored GLS specifications based on an assumption of first-order autoregressive errors, and the results are quite similar to those reported in the tables below. Rather than impose this assumption, however, we choose to estimate the model without a GLS correction, and simply

[36] Recall that we do not have a price associated with nonfuel expenses, and that according to equation (E1), fuel prices should not enter into the fuel input function. We experimented with using a variable measuring the price of a given plant's fuel relative to the prices of other fuels in the same region as an instrument for output but the variable had no power in the first stage.

[37] This use of fixed effects is similar to the work of Joskow and Schmalensee (1987), who used generating unit-level data to explore the relationship between operating performance and unit characteristics. Our

report standard errors that are corrected for a general pattern of correlation over time within a given plant.[38]

Input use over time will vary with the level of plant operation, which is measured in these specifications as the net generation by the plant in megawatt-hours (*NET MWh*). We treat the endogeneity and measurement problems described earlier by instrumenting for output with state demand (the log of total state electricity sales, a consumption rather than production measure). This instrument affects the likelihood that a given plant in the state will be dispatched more over the year, but is not influenced by the characteristics of the plant or the choices of individual plant operators.

We consider specifications that include interactions of *IOU* ownership with the three primary restructuring indicator variables described in section 3: *RESTRUCTURED, LAW PASSED,* and *RETAIL ACCESS*. In the input regressions, a negative coefficient on the restructuring variables would imply increased input efficiency associated with the regulatory reform. The core results for the input analysis are presented in Tables 4 for *EMPLOYEES*, 5 for *NONFUEL EXPENSES* and 6 for *BTU*. We first discuss the results for employment and nonfuel expenses, and then discuss the results for fuel use.

Column 1 of tables 4 and 5 reports a simple OLS formulation that excludes any control for output. In this column, *IOU*RESTRUCTURED* captures the mean differential in input use for investor-owned plants in states that eventually pass restructuring legislation, measured over the period following the first restructuring hearings, relative to IOU plants in non-restructuring states. This corresponds to the mean within-plant shift in input use, independent of output. The results suggest statistically and economically significant declines in inputs during restructuring. Employment declines by almost 6% (2%) and nonfuel expenses decline by almost 13% (2%),[39] relative to IOU plants in regimes that have not restructured. Controlling for plant output reduces the estimated impact of restructuring by more than one-quarter, though the effects remain large and statistically distinguishable from zero (see column 2 of each table), at -4% (2%) for employment and -10% (2%) for nonfuel expenses.[40] Measuring restructuring effects from the

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fixed effects are actually finer than plant-level, as we permit α_i to change with unit additions or retirements and other significant changes in rated plant capacity.

[38] Reported standard errors are calculated using the cluster option in Stata.

[39] We use $[\exp(\varphi_r^N)-1]*100$ to approximate the implied percentage effect of *IOU* RESTRUCTURED* on input use.

[40] Note that the Cobb-Douglas functional form assumption suggests that the coefficient on output should be one, substantially larger than the coefficients estimated in these regressions. If we impose this constraint, the effect of restructuring is estimated to be positive and significant. We have estimated production

enactment date of legislation does little to change the qualitative conclusions (see column 3 in each table.).

This finding hints at an interesting pattern in the data: restructuring is associated with same-plant output reductions relative to same-plant output in non-restructured regimes at the same date. This is why estimated input use reductions associated with restructuring are smaller in the presence of output controls. Some of this might be expected: if these plants were less efficient prior to restructuring, as the raw input and price data suggest may have been the case, they may be used less in a more competitive environment. If restructuring encourages entry by non-utility or exempt generators, this might further reduce output at less efficient plants.[41] Some plant-level output reduction also may be generated by the apparent lower growth in state-level electricity consumption in restructuring states, relative to electricity consumption growth in non-restructuring states.[42]

This result is not only of interest economically, but also is important for the econometric estimation of the input demand relationship. The inverse correlation of output and restructuring policy attaches great importance to obtaining a consistent estimate of the relationship between output and input demand. Understating the response of input use to output changes will load more of observed input reductions onto the restructuring variable, overstating the responsiveness will lead to underestimates of the restructuring effect.

The second notable pattern is the dependence of the implied restructuring effect on the control group. While IOU plants in restructuring states exhibit modest reductions in employment and nonfuel expenses relative to IOUs in non-restructuring states, the implied reductions are more than twice as large when compared to public and cooperative plants over the restructuring period. Employment drops by 13-15 percent (see the difference in the *IOU*RESTRUCTURED* and *MUNI*POST 1992* coefficients in columns 2 and 3 in table 4) and nonfuel expenses decline by 21-23 percent (columns 2 and 3 in table 5), relative to municipal plants over the 1993-1999

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functions in *EMPLOYEES* and *NON-FUEL EXPENSES* using more flexible functional forms than Cobb-Douglas, and the results also suggest efficiency gains associated with restructuring. We have also estimated instrumental variables versions of equations (L2) and (M2) that include the other input instead of output and obtained very similar results to those reported here.

[41]Total state-level electricity consumption, while increasing on average during the 1993-1999 period in restructuring states, appears to grow less fast than consumption in non-restructuring states.

[42] This is unlikely to be causally related to restructuring, as restructuring effects on retail rates were neutral or negative (due to rate freezes) during this period. There is, however, a negative correlation between state electricity consumption and restructuring, conditioning on state means and national growth rates in electricity consumption.

period. This suggests that even IOU plants in non-restructuring regimes were reducing their input use to a significant extent during the 1993-1999 period, perhaps in response to latent threats of increased competition and restructuring. We return to this issue in greater detail below.

Given the importance of controlling for output in assessing restructuring effects, we next turn to instrumental variables estimates of the input equations that treat potential measurement error and simultaneity bias with respect to output. Column 4 of these tables instrument for the log of current plant output with the log of total electricity sales in the state.[43] This has opposite effects on the estimated output coefficients for the two input demands. For *EMPLOYEES*, the instrumental variables (IV) estimates of the output coefficient are below the OLS estimates, although the imprecision of the IV estimates make it difficult to reject equivalence. This is consistent with there being a positive relationship between shocks to efficiency and input use, although as we discuss below, the direction of this relationship is not robust to alternative specifications of the instrument. Both OLS and IV estimates of the labor demand elasticity with respect to output are quite small (at 8.1% (1% standard error) and 6.5% (7.5%), respectively for the basic specification), suggesting that labor is relatively fixed. Consistent with this, the estimated coefficient on *IOU*RESTRUCTURED* is largely unchanged in the basic specification, at -4.5% (2.3%).

In the case of nonfuel expenses, instrumenting for output substantially increases the estimated output elasticity, from 15% (1%) to 40% (9%) in the basic specification. This is consistent with negative correlations of input shocks and output, as for example, if large maintenance expenditures are associated with outages at the plant. Given the strong link between output and nonfuel expenses implied by these results, and the fact that both declined with restructuring, instrumenting also has a substantial effect on the estimated effect of restructuring. The estimated coefficient on *IOU*RESTRUCTURED* coefficient drops by more than half, bringing it into the range of the estimated labor input effect at about -4.8% (2.8%).

Columns 5 and 6 of the tables explore robustness to alternative measures of restructuring, maintaining the use of IV estimates.[44] Measuring restructuring by *LAW PASSED* in column 5

[43] This is an important determinant of plant-level output. The F-statistic from the test that ln(*STATE SALES*) is zero in the first stage for Column (4) is F(1,9674)=18.13 for Table 4 and F(1,9686)=16.68 for Table 5).

[44] Results are broadly similar in unreported regressions that measure restructuring from the date of legislation, although the effect of *RETAIL ACCESS* is further tempered, and in regressions that add the number of years since hearings were begun, which suggest that employment and nonfuel expense reductions begin relatively small and increase over time.

yields slightly larger coefficients on restructuring in the employment regressions and slightly smaller (and statistically indistinguishable from zero) coefficients in the nonfuel expense regressions. Column 6 adds the *RETAIL ACCESS* variable. We note that its coefficient is identified by no more than two years of data in the four states that implement retail access as of 1999, and it is not particularly stable across alternative instrument sets. The coefficient on *RETAIL ACCESS* in labor demand is imprecisely estimated, though the point estimate suggests an additional –5% (5% standard error) change in employment when states implement retail access. The estimated impact of retail access on nonfuel expenses is substantially larger, at -15% (7.8%).

In the instrumental variables results, as in the OLS results, the implied magnitude of the restructuring effect depends upon the chosen benchmark or control group. The gap in IOU input demand between restructuring and non-restructuring states, conditional on output, is generally statistically and economically significant, though relatively modest. The performance gain of an IOU plant in a restructured regime relative to *MUNI* plants over the same period is substantially larger, on the order of 15% reductions in employees and 20% reductions in nonfuel expenses. We therefore experimented with a number of more flexible specifications of the *MUNI* control group. Column 7 of tables 4 and 5 reports one variant, in which we include separate controls for *MUNI* plants over the 1993-1999 period and *MUNI* plants in restructuring regimes (*MUNI*RESTRUCTURED*). In neither case can we reject the null that *MUNI* plant input use is independent of restructuring (point estimates for *MUNI*RESTRUCTURED* are positive for employees and negative for nonfuel expenses). In unreported specifications we have allowed for more flexible specifications of the *MUNI* controls over time and for differential *MUNI* output coefficients. The pattern of results is consistent with input performance gains for IOU plants in restructured regimes relative to nonrestructured IOU plants, and even greater gains when measured against *MUNI* performance.

To provide further insight into the question of benchmark group, we re-estimate the basic model of column (4) without the *IOU*RESTRUCTURED* and *MUNI*POST 1992* variables, but allowing for separate year effects for each of three categories of plants: IOU plants in states that eventually restructure, IOU plants in states that do not restructure, and *MUNI* plants. Figures 1 (employees) and 2 (nonfuel expenses) plot the estimated year effects for each plant group. Figure 1 suggests that *MUNIs* use more labor than do IOU plants, all else equal, throughout the sample period. The figures suggest greater divergence between *MUNI* and IOU plants in both input measures as the 1990s progress. As this is a period of increasing competitive pressures and substantial movement toward restructuring, these patterns suggest to us that there is considerable information in the *MUNI* benchmark comparisons.

Table 6 reports results from variants of our basic specification for fuel inputs. In column 1, the output coefficient is constrained to unity (effectively estimating ln(BTU/MWh) as a function of the right-hand side variables). In this specification, IOU plants in restructured regimes appear to use slightly more fuel than IOU plants in nonrestructured regimes, all else equal, though the effect is imprecisely estimated (1.2%, standard error 0.8%). Column 2 reports OLS results relaxing the output coefficient constraint, which generates a lower estimated output elasticity (.9, standard error .1), and an implied reduction in fuel use associated with IOU plants in restructuring regimes (-1.2%, again relatively imprecise with a standard error 0.7%). The remaining columns report results instrumenting for output with state electricity consumption. The qualitative results in all four columns are very similar—fuel use is very highly correlated with the megawatt-hours a plant produces, with an estimated output elasticity that is slightly below one though statistically indistinguishable from unity. This could reflect slight increasing returns to scale with respect to fuel, consistent with the well-understood engineering relation between more efficient fuel burn and higher levels of output at a given plant, though the data are not precise enough to establish this.

In all IV specifications, the coefficient estimates for *IOU*RESTRUCTURED* are small, positive, and statistically indistinguishable from zero. These provide no evidence of restructuring effects on fuel efficiency relative to IOU plants in non-restructuring states. Indeed, the coefficient on *RETAIL ACCESS* in column 5 implies 5% (3%) higher fuel use, all else equal, for plants operating under retail access—an enormous increase in economic terms. While somewhat higher fuel use in restructured wholesale markets could be consistent with more startup and ramping costs as plants adjust their output to follow hourly changes in marginal revenue, the estimated magnitude of this effect is too large to be explained even by this behavior.

The difference between IOU and *MUNI* plants is more difficult to pin down for fuel use than for the other inputs. The *MUNI* coefficient point estimates are slightly larger, and though they can be distinguished from zero, are not generally distinguishable from the *IOU*RESTRUCTURED* point estimates. We find little evidence of gains in fuel efficiency over our horizon from restructuring, though a caveat is in order. While variations on the order of even 0.5%-1% in fuel productivity are economically significant, it may be difficult to measure these sufficiently precisely with our aggregated data. Because fuel efficiency at a plant is heavily influenced by factors such as the allocation of output across units at a plant, the number of times its units are stopped and started, and for how long the units were running below their capacity, our inability to measure or control for possible changes in these operational characteristics may make it particularly difficult to capture fuel efficiency changes. We continue to explore this aspect of utility input choice.

Sample selection

As noted earlier, our results could be affected by sample attrition due to plant retirements and ownership transfers that eliminated data reporting. Plant sales are particularly an issue in retail access states, as exempt wholesale generators (EWGs) purchased plants that incumbents were required to divest during restructuring. Since EWGs are not regulated by FERC in the traditional sense, these companies are not required to report operating data on their plants, and hence drop out of the database. If the plants dropping out of the sample had been systematically higher or lower in efficiency relative to their plant-epoch effect and to surviving plants, selection bias could contaminate the restructuring estimates. One could construct plausible stories that plants selected for purchase were very efficient and thus good acquisition targets, or that inefficient plants left the database due to retirements or sales that offered the new owners the opportunity for substantial gain if efficiency could be improved. The correlation coefficient of the last year of data on the plant with the efficiency measures used in the regression is negative (-0.17 for employees per MW, -0.05 for non-fuel expenses per MW), suggesting that less efficient plants tend to drop out of the database, either because they are sold or shutdown. To assess the impact of attrition, we estimated both basic IV specifications and specifications that included the *RETAIL ACCESS* variable, in each case restricting the sample to plants that were in the dataset in the last year, 1999. The results for this sample (available from the authors) are for the most part very similar to the results for the full sample, suggesting that selection bias is not significant in our results. The one exception is the coefficient on *IOU*RETAIL*, which declines in magnitude and is statistically indistinguishable from zero. This may be consistent with anecdotal evidence suggesting that utilities deferred maintenance on plants they knew they were going to divest, which could be picked up with the coefficient on *IOU*RETAIL* until we limit the sample, or simply due to the difficulty of estimating the *RETAIL ACCESS* effect with so little data.[45]

Testing robustness of the RESTRUCTURED effect

[45] Imposing this selection rule eliminates, for example, almost all Massachusetts and California IOU plants due to required divestitures before 1999.

We next consider the robustness of our results to a variety of alternative specifications of the input demand equations. Given the null results in our basic fuel use regressions, we focus on labor and nonfuel expense input choices in this analysis.

In Table 7, we report results that use an alternative measure of competitive pressure, based on an indicator that turns on in 1993 if the plant is in a state that has above median penetration of non-utility generation as of 1993 (*HIGH NUGS)*. This measure should capture any utility responses to higher intensity of actual generation competition from unregulated market participants. We consider this as a replacement for the policy variables in columns 1 and 3, and as a complement to the policy variable in columns 2 and 4, which include direct effects of *RESTRUCTURED* and *HIGH NUGS* in addition to an interaction of these variables, *BOTH.* The estimated impact of high levels of non-utility generation on employment at IOU plants (columns 1) is negative and larger than the estimated effect of *RESTRUCTURED* in table 4, at -6% (3%). The effect is somewhat larger in column 2, which includes controls for restructuring, at -8% (3%), as is the estimated direct effect of restructuring (-6%, 3% standard error). It appears that these mechanisms are close substitutes, however, as the presence of *BOTH* largely offsets the effect of *RESTRUCTURED* (with a positive though insignificant effect estimated at 6%, standard error 3%). For nonfuel expenses, high NUG penetration appears to have no detectable direct effect on IOU plant input use, and the results in column 4 suggest that controlling for NUG penetration eliminates the direct measured restructured effect: Only IOU plants in regimes with both high NUG penetration and restructuring reduce nonfuel expenses relative to other IOU plants (see the coefficient on *BOTH* in column 4). In all four specifications, the estimated difference between *MUNIS* and *IOUS* is around 15%, very similar to this difference in tables 4 and 5.

In tables 8 (employees) and 9 (nonfuel expenses), we explore results for two sampling strata: large v. small plants (columns 1 and 2), and old v. new plants (columns 3 and 4). Recall that these are dimensions on which municipal plants appear to differ from IOU plants. For all specifications, IOU plants in restructured regimes exhibit lower input use than do IOU plants in nonrestructured regimes (see the coefficients on *IOU*RESTRUCTURED*), though the magnitude of the estimated effect varies with the subsample. Employee reductions appear to be largest at small and old plants; nonfuel expense reductions appear to be largest at large plants, and similar across old and new plants. More interesting, perhaps, is the comparison to *MUNI* plants. The coefficient on *MUNI*POST 1992* measures the difference between publicly-owned plants and IOUs in nonrestructuring regimes over the 1993-1999 period. While the point estimates remain positive, consistent with *MUNI* plants using more inputs than IOUs during this time, the relative performance of *MUNI*s differs across subsamples. They appear in general less inefficient for old plants (see column 3 of both tables), and are indistinguishable from IOUs in employment changes at old plants. Changes in employment at *MUNI* plants are more in line with IOUs at small plants, and for nonfuel expenses, the difference does not appear to vary with the size of the plant. While these differences are of some interest, it does not appear that the conclusions of the earlier tables with respect to the *MUNI* benchmark are substantially affected by these sample plant differences.

In table 10, we consider whether the results are explained by a regression to the mean phenomenon among IOU plants: is the gain in efficiency among plants in restructuring regimes because they had on average low productivity draws prior to restructuring, and simply return to mean efficiency over time? To examine this, we identify high and low cost plants and investigate the extent to which efficiency gains at the higher cost plants are offset by efficiency losses at low cost plants. To separate plants into "cheap" and "expensive" categories, we predict input use from a regression on data for the pre-restructuring period, 1981 – 1992. We calculate the mean residual for each plant and classify plants with mean residuals above zero as "expensive" and those below zero as "cheap." We then interact these expensive and cheap indicators with the restructuring variables, which are post-1992 and re-run the basic regression specification using these more detailed restructuring variables.[46] The results in columns 1 and 2 suggest most of the input declines relative to IOU plants in non-restructured regimes are associated with expensive IOU plants, at roughly 10% reductions in both labor and nonfuel expenses for expensive plants. The coefficients on the *IOU*RESTRUCTURED* CHEAP* interactions are economically and statistically indistinguishable from zero, contrary to mean reversion predictions, and perhaps suggesting that the form of efficiency improvement was to bring less efficient plants into line with more efficient plants. This is consistent with comments we have received from several industry participants, who claimed that restructuring led their firms to identify high-cost plants as those disadvantaged in the dispatch order, and to focus attention on bringing the costs of those plants closer to an efficient benchmark plant. Using the *MUNI* benchmark implies larger efficiency gains for both cheap and expensive IOU plants, as both categories of *MUNI* plants experienced higher input use than IOU plants in nonrestructured states, though the increase was largest at the cheap *MUNI* plants.

We have implemented a number of additional robustness checks, including alternative instruments and instrument strategies and more flexible dynamics in input choice. A more complete discussion and example results are available in a technical appendix. Of particular note

[46] The direct effects of cheap and expensive are absorbed in the plant fixed effects.

were specifications that allow for the possibility of fixed costs of input adjustments. We find that the restructuring estimates are robust to allowing for differential effects of output on input use depending on whether current output is above or below plant mean output, and to allowing inputs to respond to future as well as current output levels. Labor use estimates were less robust to using lagged values of output as instruments, following Blundell and Bond (1998, 2000), although the estimated difference between IOU plants in restructuring regimes and *MUNIs* remained economically important.[47]

5. Conclusion

This research provides some of the first estimates of the impact of electricity generation sector restructuring in the United States on plant-level efficiency. The results suggest restructuring may yield substantive medium-run efficiency gains. The estimates suggests that IOU plants in restructuring regimes reduced their labor and nonfuel operating expenses by about 5% in anticipation of increased competition in electricity generation relative to IOU plants in states that did not restructure their markets. The estimated efficiency gains are even larger when compared to a benchmark based on municipal, federal, and cooperative plants, on the order of 15% reductions in labor use and 20% reductions in nonfuel operating expenses relative to non-IOU plants over the same time period. There is little evidence of increases in fuel efficiency relative to plants in non-restructuring regimes, although the power of these tests is limited given the plausible range of possible fuel use improvements.

These same-plant reductions in input use suggest an important role for competition in promoting technical efficiency, buttressing the findings of Nickell (1996), Ng and Seabright (2001), and Galdón-Sánchez and Schmitz (2002), among others. This finding is particularly interesting given the industry context. Generating plant technology is reasonably well understood by engineers, and the pre-restructuring industry was remarkably open in sharing detailed information on plant operations and input use across plants and firms.[48] Presumably, external benchmarks were more accessible in this setting than in most industries. This could suggest that competition induced greater effort on cost reduction by increasing the sensitivity of returns to managerial and worker effort, rather than by reducing informational asymmetries over managerial effort (see Nickell, 1996).

[47] Whether lagged output is a valid instrument for this application is, moreover, a debatable proposition.
[48] Evidenced, for example, by our use of publicly available, plant-identifiable, data in our analysis.

Additional work remains to be done to fill out the picture of the overall effects of restructuring on electricity industry efficiency.[49] We have started by looking at operating efficiency within existing utility plants both because this is one of the few places where gains are likely to show up before restructured wholesale markets open up and because rich data are available on utility-owned plants. As our results suggest, even these data are inadequate for the fine-level analysis required to estimate within and across-plant changes in fuel efficiency. This analysis will require datasets with both cleaner measures of fuel efficiency and richer information on independent factors that affect fuel use. Finally, assessing whether investment decisions are made more efficiently after restructuring requires more time, and access to better non-utility data. Since power plants are so long-lived, very few new additions are made each year, and currently we have no more than a handful of anecdotes about investment after restructuring.

It is important to recognize that these efficiency estimates are, however, only one input to judging the ultimate benefit of restructuring policies. The overall assessment depends as well on the realized magnitude of potential dynamic efficiencies, and possible offsetting effects from higher investment expenditures, restructuring costs, the loss of coordination and network economies within vertically integrated systems, and the exercise of market power in unregulated generation markets. Dynamic costs could be higher if restructuring reduces knowledge sharing that affects productivity growth over time. It is possible, however, that longer run effects will be more striking as firms respond to the new incentives created by restructuring with investments in both human and physical capital that further enhance efficiency. If California's crisis does not induce reversals of the restructuring movement, and regulators do not shut down data reporting and researcher access to detailed plant-level data, time may enable us to distinguish among these possibilities.

[49] See Wolfram (2004) for a discussion of the general issues involved in assessing different types of efficiency changes accompany electricity restructuring.

Table 1: Number of Plants Affected by Restructuring in Each Year

Year	Total Plants	*IOU* RESTRUCTURED*	*IOU*RETAIL ACCESS*	*MUNI* POST 1992*
1981	465	0	0	0
1982	490	0	0	0
1983	511	0	0	0
1984	527	0	0	0
1985	546	0	0	0
1986	547	0	0	0
1987	582	0	0	0
1988	588	0	0	0
1989	592	0	0	0
1990	597	0	0	0
1991	600	0	0	0
1992	591	0	0	0
1993	602	27	0	119
1994	598	103	0	129
1995	608	166	0	129
1996	589	187	0	119
1997	569	239	0	101
1998	557	236	47	101
1999	486	183	30	91
TOTAL	10645	1,141	77	789

Note: *RESTRUCTURED* indicated those that have started formal hearings that eventually go on to pass legislation, and *RETAIL ACCESS* indicates plants in states that have started retail access.

Table 2a: Summary of Data for Plants Larger Than 100 MW, 1985: Restructured versus Non-Restructured IOUs

Variable	*RESTRUCTURED* (N = 244)		*NON-RESTRUCTURED* (N = 196)		Difference in Means	T-statistic for Difference
	Mean	Std. Dev.	Mean	Std. Dev.		
EMPLOYEES / MW	0.29	0.21	0.26	0.14	.03	1.79
NONFUEL EXPENSE/ MW	19780	14354	16490	8496	3291	2.99**
HEAT RATE	11	2.0	11	3.1	-.1	-.54
CAPACITY FACTOR	0.39	0.21	0.41	0.20	-.01	-.54
MW	806	662	806	645	.8	0.01
AGE	28	11	24	13	3.4	2.95**
% COAL	52	50	80	40	-28	-6.48**
% GAS	36	48	16	37	21	5.11**

Table 2b: Summary of Data for Plants Larger Than 100 MW, 1985: Munis versus Non-Restructured IOUs

Variable	*MUNIS* (N=106)		*NON-RESTRUCTURED* (N = 196)		Difference in Means	T-statistic for Difference
	Mean	Std. Dev.	Mean	Std. Dev.		
EMPLOYEES / MW	0.27	0.13	0.26	0.14	.01	.45
NONFUEL EXPENSE/ MW	15220	9292	16490	8496	-1269	-1.17
HEAT RATE	12	5.9	11	3.1	.4	.68
CAPACITY FACTOR	0.40	0.23	0.41	0.20	-.01	-.23
MW	703	608	806	645	-102	-1.36
AGE	19	11	24	-5	-4.8	-3.53**
% COAL	68	47	80	40	-12	-2.16**
% GAS	27	45	16	37	12	2.27**

** Significant at .05 level or better

Table 3a: Percentage Change in Costs From 1990 to 1996
Difference of Means Tests
IOUs in Non-Restructuring Versus Restructuring States

	N	Distribution	Transmission	Generation
Restructuring Mean	74	12.3%	22.7%	-17.0%
Non-Restructuring Mean	49	17.3%	33.8%	4.2%
Difference		-5.0%	-11.1%	-21.3%
t stat		-0.99	-1.34	-3.65**

*significant at 5% level   **significant at 1% level
All measures are in nominal dollars.

Table 3b: Percentage Change in Costs Per MWh[#] From 1990 to 1996
Difference of Means Tests
IOUs in Non-Restructuring Versus Restructuring States

	N	Distribution	Transmission	Generation
Restructuring Mean	72	1.5%	13.1%	-13.5%
Non-Restructuring Mean	48	-1.6%	12.6%	-5.1%
Difference		3.1%	0.4%	-8.3%
t stat		0.70	0.06	-1.87

*significant at 5% level   **significant at 1% level
All measures are in nominal dollars.

[#] Transmission and Distribution costs per MWh are costs per MWh sales to ultimate customers, while Generation costs per MWh are costs per MWhs generated at company plants.

Note: Three observations dropped due to lack of MWh data.

Table 4: Base Specifications, ln(*EMPLOYEES*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS **No MWh**	**OLS** **Basic**	**OLS** **Law Date**	**IV** **Basic**	**IV** **Law Date**	**IV** **Retail Access**	**IV** **Muni* Restructured**
IOU*RESTRUCTURED	-0.059***	-0.042**		-0.045**		-0.050**	-0.047**
	(0.019)	(0.019)		(0.023)		(0.022)	(0.024)
IOU*LAW PASSED			-0.059***		-0.066***		
			(0.020)		(0.024)		
IOU*RETAIL ACCESS						-0.051	
						(0.051)	
MUNI*POST 1992	0.091***	0.095***	0.103***	0.095***	0.103***	0.093***	0.087***
	(0.026)	(0.025)	(0.025)	(0.025)	(0.025)	(0.024)	(0.028)
MUNI*RESTRUCTURED							0.034
							(0.045)
ln(WAGE)	-0.119***	-0.098**	-0.104***	-0.102**	-0.114**	-0.112**	-0.107**
	(0.041)	(0.039)	(0.039)	(0.044)	(0.045)	(0.047)	(0.045)
ln(NET MWH)		0.081***	0.081***	0.065	0.047	0.041	0.054
		(0.010)	(0.010)	(0.075)	(0.073)	(0.081)	(0.079)
AGE	0.003	0.003	0.003	0.003	0.003	0.003	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
FGD	0.065	0.081*	0.085	0.081	0.079	0.073	0.074
	(0.053)	(0.048)	(0.052)	(0.051)	(0.056)	(0.052)	(0.051)

N = 10634; 956 Plant-epoch and 19 year effects included.

Robust clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Instrument for *ln(NET MWH)*: *ln(STATESALES)*

Table 5: Base Specifications, ln(*NONFUEL EXPENSES*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS No MWh	**OLS Basic**	**OLS Law Date**	**IV Basic**	**IV Law Date**	**IV Retail Access**	**IV Muni* Restructured**
IOU*RESTRUCTURED	-0.135***	-0.103***		-0.048*		-0.046*	-0.049*
	(0.022)	(0.021)		(0.028)		(0.028)	(0.029)
IOU*LAW PASSED			-0.099***		-0.038		
			(0.025)		(0.031)		
IOU*RETAIL ACCESS						-0.167**	
						(0.075)	
MUNI*POST 1992	0.131***	0.138***	0.164***	0.149***	0.161***	0.147***	0.155***
	(0.028)	(0.026)	(0.026)	(0.027)	(0.026)	(0.027)	(0.028)
MUNI*RESTRUCTURED							-0.030
							(0.048)
ln(NET MWH)		0.146***	0.148***	0.399***	0.417***	0.382***	0.396***
		(0.013)	(0.013)	(0.090)	(0.088)	(0.094)	(0.092)
AGE	-0.001	-0.002	-0.002	-0.002	-0.003	-0.002	-0.002
	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)
FGD	0.171***	0.201***	0.209***	0.253***	0.260***	0.250***	0.252***
	(0.032)	(0.032)	(0.030)	(0.047)	(0.046)	(0.046)	(0.047)

N = 10645; 956 Plant-epoch and 19 year effects included.

Robust clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Instrument for *ln(NET MWH)*: *ln(STATESALES)*

Table 6: Base Specification, ln(*BTU*)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS $B_{ln(NET\ MWH)}$=1	OLS	IV Basic	IV Law Date	IV Retail Access	IV Muni* Restructured
IOU*RESTRUCTURED	0.012	-0.013*	0.005		0.005	0.008
	(0.008)	(0.007)	(0.014)		(0.014)	(0.014)
IOU*LAW PASSED				0.008		
				(0.011)		
IOU*RETAIL ACCESS					0.053*	
					(0.029)	
MUNI*POST 1992	0.032*	0.027*	0.031*	0.030*	0.031*	0.015
	(0.017)	(0.016)	(0.016)	(0.017)	(0.016)	(0.012)
MUNI*RESTRUCTURED						0.082*
						(0.048)
ln(NET MWH)		0.888***	0.971***	0.971***	0.976***	0.979***
		(0.010)	(0.042)	(0.037)	(0.045)	(0.041)
AGE	0.002	0.002	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
FGD	0.011*	-0.012	0.005	0.005	0.006	0.007
	(0.007)	(0.010)	(0.011)	(0.009)	(0.011)	(0.011)

N = 10645; 956 Plant-epoch and 19 year effects included. Robust clustered standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%
Instrument for *ln(NET MWh)*: ln(*STATESALES*)

Table 7: Non-Utility Generators (NUG) Results, IV Estimates

	(1)	(2)	(3)	(4)
	IV *ln(EMPLOYEES)*	IV *ln(EMPLOYEES)*	IV *ln(NON-FUEL EXPENSES)*	IV *ln(NON-FUEL EXPENSES)*
IOU*HIGH NUGS	-0.064**	-0.081***	-0.015	0.037
	(0.027)	(0.027)	(0.035)	(0.036)
IOU*RESTRUCTURED		-0.065**		-0.004
		(0.028)		(0.037)
IOU*BOTH		0.054		-0.063
		(0.034)		(0.044)
MUNI*POST 1992	0.087***	0.072***	0.156***	0.157***
	(0.025)	(0.026)	(0.028)	(0.030)
ln(WAGE)	-0.085**	-0.095**		
	(0.043)	(0.044)		
ln(NET MWH)	0.053	0.012	0.438***	0.503***
	(0.076)	(0.086)	(0.092)	(0.103)
AGE	0.003	0.003	-0.002	-0.002
	(0.002)	(0.002)	(0.005)	(0.005)
FGD	0.079**	0.068	0.258***	0.274***
	(0.039)	(0.052)	(0.057)	(0.056)
N	10634	10634	10645	10645

956 Plant-epoch and 19 year effects included.
Robust clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%
Instruments for *ln(NET MWh)*: ln(*STATESALES*)

Table 8: Results by Plant Characteristic, *ln(EMPLOYEES)*

	IV		IV	
	BIG	**SMALL**	**OLD**	**NEW**
IOU*RESTRUCTURED	-0.030	-0.076**	-0.061*	-0.032
	(0.026)	(0.036)	(0.036)	(0.032)
MUNI*POST 1992	0.062*	0.106***	0.021	0.111***
	(0.036)	(0.033)	(0.040)	(0.030)
ln(WAGE)	-0.030	-0.194***	-0.148*	-0.097*
	(0.057)	(0.068)	(0.077)	(0.050)
ln(NET MWH)	0.028	0.044	0.150	-0.042
	(0.116)	(0.093)	(0.138)	(0.101)
AGE	<0.001	0.003	0.001	-0.103*
	(0.003)	(0.003)	(0.004)	(0.054)
FGD	0.151***	-0.005	0.061	0.153***
	(0.038)	(0.035)	(0.074)	(0.026)
Observations	5325	5309	5061	5573

Plant-epoch and year fixed effects included.

Robust clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: Results by Plant Characteristic, *ln(NONFUEL EXPENSES)*

	IV		IV	
	BIG	**SMALL**	**OLD**	**NEW**
IOU*RESTRUCTURED	-0.074**	-0.044	-0.047	-0.053
	(0.033)	(0.038)	(0.044)	(0.035)
MUNI*POST 1992	0.096**	0.180***	0.117***	0.159***
	(0.038)	(0.036)	(0.041)	(0.034)
ln(NET MWH)	0.462***	0.315***	0.448***	0.355***
	(0.164)	(0.084)	(0.162)	(0.108)
AGE	-0.011***	<0.001	-0.008	0.016
	(0.004)	(0.005)	(0.006)	(0.042)
FGD	0.253***	0.256***	0.301***	0.085**
	(0.059)	(0.034)	(0.071)	(0.034)
Observations	5327	5318	5063	5582

Plant-epoch and year fixed effects included.
Robust clustered standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 10: Test for Mean Reversion Effect, IV Estimates

	ln(EMPLOYEES)	*ln(NON-FUEL EXPENSES)*
***IOU*RESTRUCTURED*_cheap**	-0.011	-0.004
	(0.028)	(0.022)
***IOU*RESTRUCTURED*_expen**	-0.106***	-0.102***
	(0.031)	(0.022)
***MUNI*POST 1992*_cheap**	0.143***	0.193***
	(0.041)	(0.022)
***MUNI*POST 1992*_expen**	0.056**	0.095***
	(0.027)	(0.021)
ln(WAGE)	-0.115***	
	(0.045)	
ln(NET MWH)	0.037	0.409***
	(0.084)	(0.061)
AGE	0.003	-0.003
	(0.002)	(0.003)
FGD	0.028	0.235*
	(0.056)	(0.123)
	10222	10258

842 plant-epoch and 19 year fixed effects included.
Robust clustered standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%
Instruments for *MWh*: ln(*STATESALES*)

Figure 1: Year-Effects by Group from Basic IV Specification: Employment

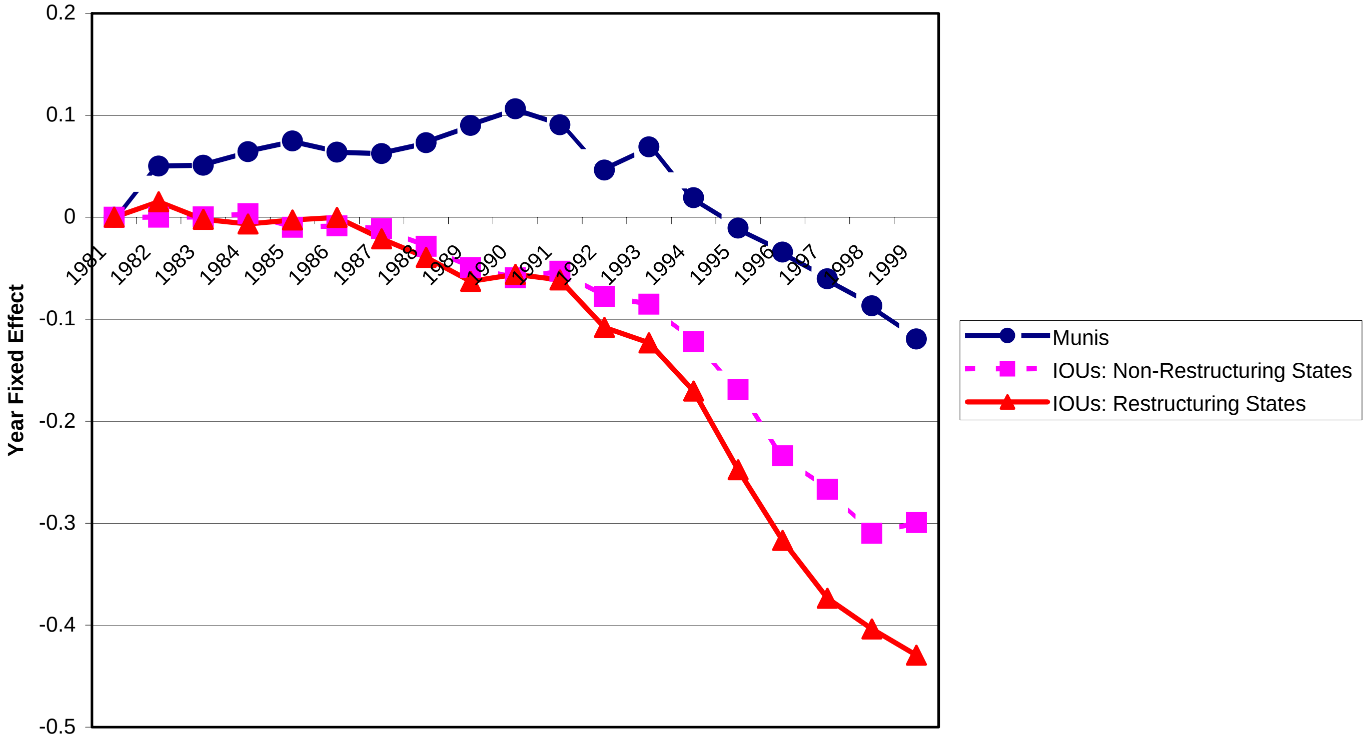
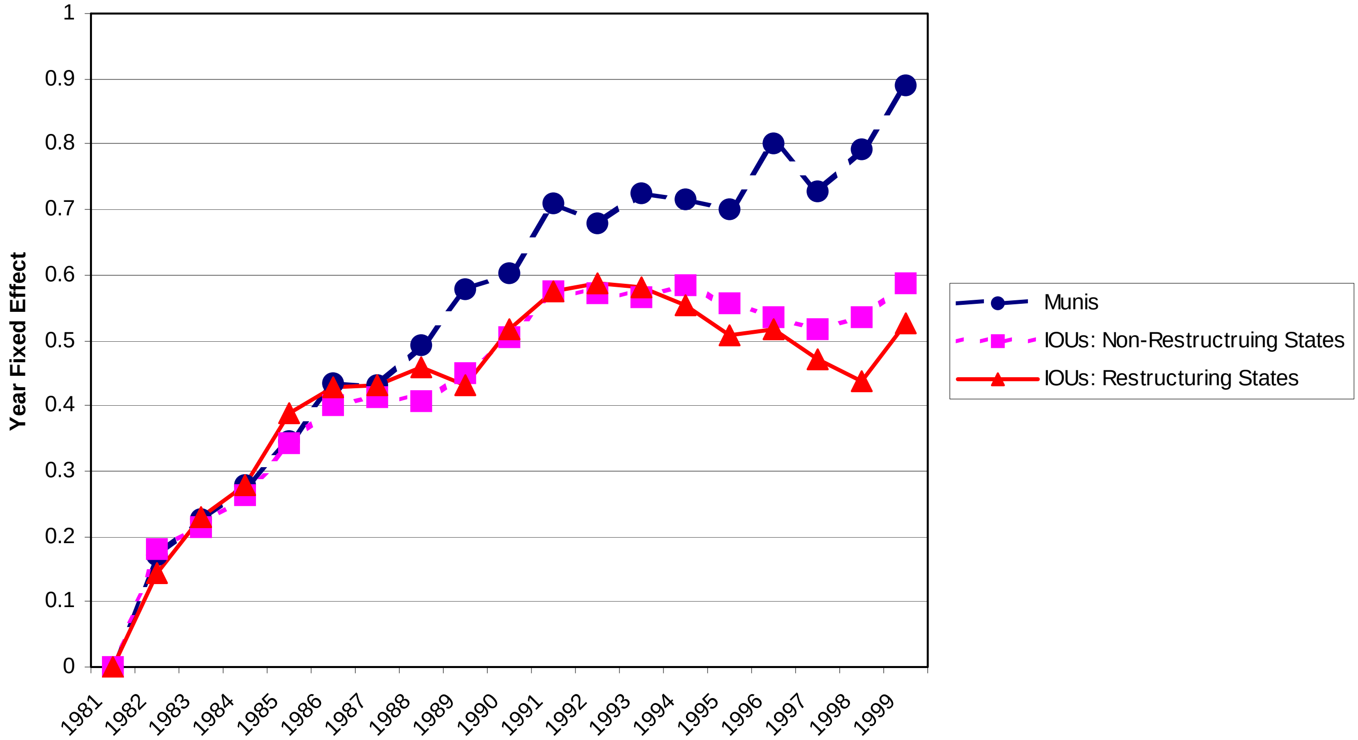


Figure 2: Year-Effects by Group from Basic IV Specification: Non-fuel Expenses



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Data Appendix – Table A1: Summary of Variables

Variable	Definition	Source
Output and Input Variables		
LN_NONFUEL EXPENSE	Log of the annual non fuel production expenses ($)	UDI
LN_EMPLOYEES	Log of the average number of employees at the plant.	UDI
LN_BTU	Log of the total btus of fuel burned by the plant. Calculated as (tons of coal * 2000 lbs/ton* btu/lb) + (barrels of oil*42 gal/barrell*btu/gal) + (Mcf gas*1000 cf /mcf*btu/cf). The values of the btu content of each fuel are provided in the UDI data for each plant each year.	UDI
LN_NET MWH	Log of the net MWh generation of the plant.	UDI
Restructuring Variables		
IOU	Dummy equal to 1 for plants classified as IOU, holding, or private companies.	1997 UDI Utility Datapak Book
MUNI	Dummy equal to 1 for plants owned by utilities classified as government or cooperative utilities.	1997 UDI Utility Datapak Book
*IOU*RESTRUCTURED*	Dummy equal to one for IOU plants in states that restructured in years following the advent of formal hearings.	Restructuring information compiled from: EIA "Status of State Electric Industry Restructuring Activity" Timeline as of July 2002, EIA "The Changing Structure of the Electric Power Industry: An Update, 12/96," EEI "Electric Competition in the States" February, 2001, EIA "The Changing Structure of
*MUNI*RESTRUCTURED*	Dummy equal to one for MUNI plants in states that restructured in years following the advent of formal hearings	
*IOU*LAW PASSED*	Dummy equal to one for IOU plants in states that restructured, beginning with year that legislation was enacted	
*IOU*RETAIL ACCESS*	Dummy equal to one for IOU plants in states that restructured in years following the advent of retail access.	

*MUNI*POST 1992*	Dummy equal to one for MUNI plants in years 1993-1999.	the Electric Power Industry: 2000 An Update," NARUC "Utility Regulatory Policy in the United States and Canada, Compilation" 1994 - 1995 and 1995 – 96, "Restructuring the Electricity Industry," The Council of State Governments, 1999, and State PUC websites, relevant legislation and reports.
*IOU*HIGH NUGS*	Dummy equal to one beginning in 1993 for IOU plants in states with above median penetration of NUG plants in 1993.	EIA Electric Power Annual.
BOTH	Interaction of RESTRUCTURED and HIGH NUGS dummy variables.	
CHEAP(EXPENSIVE)	Dummy indicating whether plant average residual from input use regression for 1981-1992 period is below (above) zero.	
INDEPENDENT / OTHER VARIABLES		
PLANT AGE	Number of years since the oldest unit at the plant came on line.	UDI
FGD	Dummy equal to 1 indicating the operation of a FGD scrubber at the plant.	UDI
CAPACITY FACTOR	Capacity factor of the plant, calculated as (*NET MWH*) / (*GROSS MW* * hours in year)	UDI
LN_WAGE	State-level annual wage bill divided by total employment at plants in the following industries: SIC 4923-4925 (natural gas distribution), 4953 (hazardous waste treatment), 2911 (petroleum refining).	BLS
STATE SALES	State-level utility sales in gigawatthours, not including non-utility sales.	Sales: Sales to Ultimate Customers from EIA's "Electric Sales and Revenue" Table 6 and EIA's

		"Electric Power Annual" Tables 117 and 90.
PLANT CHARACTERISTIC VARIABLES		
LOAD TYPE	Based on the load type reported in FERC Form 1 through 1992. We assign plants to the last load type reported if they change load type no more than twice, otherwise we do not assign load types to missing data years. We use this cutoff to include plants had a "stray" change--one peak, cycle or shutdown type in the middle of a string of "base" types, for example.	UDI
FUEL TYPE	Based on the primary fuel burned at the plant	UDI
BIG	Dummy variable equal to one if the plant capacity (Gross MW) is greater than 550MW.	UDI
OLD	Dummy equal to one if the youngest unit at the plant came in service before 1960.	UDI
ECONOMIC AND WEATHER VARIABLES		
ANNUAL_UNEMPLOYMENT	Annual average unemployment in the state.	Bureau of Labor Statistics
ANNUAL_HDDAYS	Population weighted heating degree days for each state-year. (use MD for DC)	U.S. Dept. of Commerce National Oceanic and Atmospheric Administration Historical Climatology Series, "Monthly State, Regional, and National Heating Degree Days Weighted By Population"
ANNUAL_CDDAYS	Population weighted cooling degree days for each state-year. (use MD for DC)	

Note: UDI stands for the Utility Data Institute O&M Production Cost Database. All data in the UDI production cost data bases are extracted from the FERC Form 1 (filed by investor-owned utilities), EIA Form 412 (filed by municipal and other government utilities), and RUS Form 7 & 12 (filed by electric cooperatives). These are annual reports.

Data Appendix - Table A2: Summary Statistics

Variable	Obs	Mean	Std.Dev.	Min	Max
LN_NONFUELEXP	10645	15.880	1.137	9.674	18.694
LN_EMPLOYEES	10645	4.562	1.108	0	7.099
LN_BTU	10645	30.304	1.631	19.839	33.067
LN_NET MWH	10645	14.075	1.760	5.476	16.905
PLANTAGE	10645	27.925	12.940	0	76.000
FGD	10645	0.124	0.330	0	1
LN_WAGE	10645	10.543	0.255	9.815	11.488
IOU	10645	0.805	0.396	0	1
MUNI	10645	0.195	0.396	0	1
*IOU*RESTRUCTURED*	10645	0.107	0.309	0	1
*MUNI*RESTRUCTURED*	10645	0.013	0.113	0	1
*IOU*LAW PASSED*	10645	0.081	0.272	0	1
*IOU*RETAIL ACCESS*	10645	0.007	0.085	0	1
MUNI POST 1992*	10645	0.074	0.262	0	1
*IOU*HI NUGS*	10645	0.103	0.304	0	1
BIG	10645	0.500	0.500	0	1
OLD	10645	0.476	0.499	0	1
LN_STATE SALES	10645	11.171	0.850	8.285	12.627
ANNUAL_UNEMP	10645	0.064	0.021	0.022	0.180
ANNUAL_CDDAYS	10608	1439.436	931.661	81	3879
ANNUAL_HDDAYS	10608	4382.568	2144.882	414	10758

Technical Appendix: Sensitivity Analysis

Robustness of results to alternative instruments

The conclusion that same-plant input use declined after restructuring for IOU plants appears robust to a variety of alternative specifications, although the precise magnitude of the estimated efficiency gain depends crucially on the estimated relationship between inputs and output. In tables T1 and T2, we analyze the sensitivity of the results to alternative output specifications and instrument sets, using the basic IV specification of the regression model (similar to column 4 of tables 4 and 5).

In the first column of each table, we report results that interact the coefficient on output with variables indicating whether the plant achieved a high or low output relative to its historical performance. We use a cut-off between high and low output levels based on the 60th percentile of the plant's historical output, although specifications using cutoffs ranging from the 10th to the 80th percentile yielded qualitatively similar results. Introducing this flexibility could be important if fixed labor and materials costs cannot be scaled back when the plant operates at historically low output levels. The results in column (1) suggest that the relationship between output and employment (table T1) or output and nonfuel expense (table T2) is not materially different across output levels, however.

Columns 2 through 4 of tables T1 and T2 introduce an array of nonlinearities in the instrument set. In column (2), we add the square of ln(*STATESALES*) as an instrument. This allows extreme departures from state average sales to have an additional predictive effect on plant output.[50] Column (3) adds an interaction term between ln(*STATESALES*) and *RETAIL ACCESS*, allowing the relation between state demand and plant output to vary after implementation of retail access.[51] Column (4) interacts ln(*STATESALES*) with fuel type indicator variables, allowing the plant output to respond differentially to state demand depending upon fuel type. For employment, reported in table T1, these instrument sets almost all increase the estimated magnitude of the coefficient on ln(*NET MWhs*) relative to the specifications reported in Table 4. The restructuring effect remains negative and statistically significant, although smaller than reported in Table 4. For non-fuel expenses, reported in table T2, these changes in the instrument set have varying effects on the estimated magnitude of the output elasticity as well as the impact of restructuring, relative to results in Table 5. Efficiency effects remain large relative to municipal plants in all specifications, and roughly the

[50] In the first stage, the coefficient on state sales is significant, although the F-statistic on the hypothesis that both ln(*STATESALES*) and ln(*STATESALES*)^2 are equal to zero is smaller than the F-statistic from a specification that just uses ln(*STATESALES*).

magnitude of those found in table 5 with respect to IOU plants in non-restructuring regimes, with the exception of results in column (3).

The final two columns of tables T1 and T2 use lagged values of output as instruments. This follows discussions in Blundell and Bond (1998, 2000) on the use of lagged values of endogenous variables in applications to input demand and production function specifications, particularly in the case of weak exogenous instruments.[52] In our case, lagged values of output are very highly correlated with output in all specifications, even those estimated using first differences, although first differences may not be appropriate given that most plants have scheduled major outages for equipment maintenance and overhauls on a two to four year cycle. We explore two variations. The simplest, in column (5), uses the prior year output as an instrument. We report results from this as a base, though this is valid only if there is no first-order serial correlation in the time-varying efficiency shock, unlikely given the estimated rho in our data. The second variation uses lags from years two through four years before the current. For employment, this instrument set substantially reduces, or eliminates (Table 11, column 6) the estimated efficiency gain of restructuring relative to nonrestructuring IOU plants, though the performance gain relative to *MUNI*s remains large. The column (6) results also suggest a coefficient on output that is much higher than in any other specifications. In the case of non-fuel expenses, these instruments suggest results for restructuring effects that are broadly consistent with our other results.

Robustness of results to including forward-looking output

We have explored specifications that allow for a fixed cost of changing input levels, particularly employment.[53] This could cause a plant that foresaw declining output over a several year period, for instance accompanying restructuring, to ratchet employment in a discrete change. If this were the case, the plant would appear especially unproductive prior to an employment reduction, and especially productive following the reduction until the lower output levels are realized. To investigate the effect of dynamic considerations on our results, we estimated input demand equations that included variables measuring the forward looking changes in output.[54] The coefficient on the forward looking changes in output were positive and statistically significant in the OLS specifications similar to those in columns (3) of tables 4 and 5, but

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[51] In the first stage, the coefficient on the interaction term is highly significant.

[52] In fact, Blundell and Bond describe instrumenting with lagged values in a common factor model that allows for a first-order auto-regressive term and is first-differenced to eliminate firm-specific effects. We have estimated this form of the model and the results seem to yield an AR(1) term that is over 1.6. For the reasons discussed in the text, we don't think our data have a simple first-order autoregressive error structure.

[53] There is an extensive labor literature on the presence of fixed costs and other non-convexities in employment decisions.

negative and statistically indistinguishable from zero once we instrumented for both output and forward changes in output. In both cases, the coefficients on our restructuring variables were virtually unchanged.

[54] This input demand equation is based on one derived by Rota (2004), who specifies a dynamic model where firms face a fixed cost to changing their employment levels.

Table T1: Results Using Alternative Instruments – ln(*EMPLOYEES*)

	(1)	(2)	(3)	(4)	(5)	(6)
Instruments:	**ln(*STATESALES*) spline**	**ln(*STATESALES*), ln(*STATESALES*)^2**	**ln(*STATESALES*) ln(*STATESALES*) *RETAIL ACCESS**	**ln(*STATESALES*) X Fuel-type**	**ln(*NET MWhs*)$_{t-1}$**	**ln(*NET MWhs*)$_{t-2}$, ln(*NET MWhs*)$_{t-3}$, ln(*NET MWhs*)$_{t-4}$**
IOU* RESTRUCTURED	-0.050**	-0.041*	-0.050**	-0.035	-0.028	0.007
	(0.022)	(0.024)	(0.020)	(0.022)	(0.019)	(0.022)
MUNI*POST 1992	0.091***	0.094***	0.093***	0.095***	0.080***	0.086***
	(0.025)	(0.024)	(0.024)	(0.024)	(0.021)	(0.022)
ln(WAGE)	-0.107**	-0.093**	-0.108**	-0.087**	-0.087**	-0.083
	(0.045)	(0.044)	(0.043)	(0.042)	(0.040)	(0.051)
ln(NET MWH)		0.094	0.048	0.121**	0.152***	0.330***
		(0.083)	(0.057)	(0.058)	(0.017)	(0.075)
ln(NET MWH) *HIGH CF	0.026					
	(0.118)					
ln(NET MWH) *LOW CF	0.024					
	(0.122)					
Observations	10634	10634	10634	10634	9377	6573

Plant-epoch and year fixed effects included.

Robust clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table T2: Results Using Alternative Instruments – ln(*NON-FUEL EXPENSES*)

	(1)	(2)	(3)	(4)	(5)	(6)
Instruments:	**ln(*STATESALES*) spline**	**ln(*STATESALES*), ln(*STATESALES*)^2**	**ln(*STATESALES*), ln(*STATESALES*) **RETAIL ACCESS***	**ln(*STATESALES*) X Fuel-type**	**ln(*NET MWhs*)$_{t-1}$**	**ln(*NET MWhs*)$_{t-2}$, ln(*NET MWhs*)$_{t-3}$, ln(*NET MWhs*)$_{t-4}$**
***IOU**	-0.059**	-0.051*	-0.024	-0.056**	-0.083***	-0.061**
RESTRUCTURED	(0.029)	(0.027)	(0.028)	(0.026)	(0.021)	(0.025)
MUNI*POST 1992	0.170***	0.147***	0.148***	0.146***	0.123***	0.115***
	(0.029)	(0.026)	(0.028)	(0.026)	(0.025)	(0.027)
ln(NET MWH)		0.398***	0.528***	0.374***	0.271***	0.445***
		(0.083)	(0.073)	(0.078)	(0.023)	(0.098)
***ln(NET MWH)**	0.570***					
HIGH CF	(0.152)					
ln(NET MWH)	0.586***					
****LOW CF***	(0.157)					
Observations	10645	10645	10645	10645	9386	6580

Plant-epoch and year fixed effects included.
Robust clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%