Heterogeneous Effects of Deregulation on Fuel Procurement Costs: Evidence from U.S. Electricity Restructuring

Jin Soo Han[1], Jean-François Houde[2,4],
Arthur A. van Benthem[3,4], Jose Miguel Abito[3]*

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Abstract

This paper presents new quantitative evidence of the sources of efficiency benefits from deregulation. In particular, we estimate the heterogeneous effects of plant divestitures on fuel procurement costs during the restructuring of the U.S. electricity industry. Guided by economic theory, we focus on three mechanisms and find that deregulation reduced fuel procurement costs for firms that (i) were not subject to earlier incentive programs, (ii) had relatively strong bargaining power as coal purchasers after deregulation, and (iii) were locked in with disadvantaged coal contracts prior to deregulation.

Keywords: electricity, deregulation, divestiture, incentive regulation, negotiation, bargaining power, difference-in-differences.

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Affiliations: [1]Korea Air Force Academy. [2]Department of Economics, University of Wisconsin-Madison. [3] The Wharton School, University of Pennsylvania. [4]National Bureau of Economic Research.

1 Introduction

At the turn of the 21st century, the U.S. electricity industry faced a dramatic transformation in every segment of the sector: generation, transmission, and distribution. A sizable number of states passed laws to force utilities to divest generation assets and compete in wholesale markets, transmission became the responsibility of independent system operators, and retail competition was introduced in a number of states. In the following decades, various impacts of these reforms were studied, such as competitiveness in wholesale markets, plant-level efficiency, generation investment, and retail competition (Bushnell, Mansur, and Novan, 2017). Yet, of the key interests—both to researchers and policymakers—has been the effect of power plant divestitures on fuel procurement costs (Borenstein and Bushnell, 2015).

While the fuel costs composed the largest portion of plants' operating expenses, the impact of restructuring on the fuel costs was uncertain. On the one hand, restructuring was expected to deliver reasonable reductions in fuel expenditure. Upon divestiture, power plants would compete in a competitive wholesale market and could no longer pass through such costs. On the other hand, plants' cost-minimization incentives were not absent under the traditional regulation. Roughly 50% of divested plants were already subject to some form of incentive regulation, which prior studies found was successful at achieving cost reductions by making regulated plants the residual claimants on fuel cost savings (Knittel, 2002). Moreover, throughout the 1980s and 1990s, the terms of fuel contracts were publicly available and state regulators were able to benchmark contracts with those of plants in neighboring states. Thus, the prudence of fuel procurement was readily verifiable by regulators, which should, in theory, reduce the scope of the moral hazard problem relative to other activities, such as maintenance and labor costs.[1] Because utilities faced different incentives regulated by different state authorities, not only whether but also *how* restructuring would provide additional cost-saving incentives remained an important but unanswered empirical question as encapsulated by Bushnell, Mansur, and Novan (2017)'s inquiry: "would similar plant level efficiency gains have been achieved by instituting incentive based regulation as opposed to restructuring?"[2]

To gauge the extent to which the U.S. restructuring experience can extrapolate to other settings, it is crucial to understand the underlying mechanisms that the reform operates on, identify potential heterogeneous effects of the reform at the firm-level, and confirm if they are consistent with economic theory. This paper adds new insights into how a power plant divestiture can induce fuel cost savings. Our focus is on coal procurement. We investigate the extent to which a plant's pre-divestiture coal contract differentially affects the gains from market restructuring on the cost of coal. We identify three important channels that influence the impact of divestiture: the incentives that a plant faced prior to divestiture, the degree of the buyer's bargaining leverage measured through the size of the buyer, and

[1] Fabrizio, Rose, and Wolfram (2007) report that, holding output fixed, divested plants were able to reduce labor and non-fuel expenditures by an amount ranging from 3% to 12% depending on the expenditure category and the identity of the control group.

[2] The average impact of power plant divestiture in fuel cost reduction was first studied by Cicala (2015). He finds that coal-fired power plants achieved, on average, 12% fuel cost reductions upon divestiture. In a recent paper, we document that the result was driven by a group of outliers owned and operated by a single utility and that the estimated effect becomes 6% and statistically insignificant once the outliers are accounted for (Han, Houde, van Benthem, and Abito, 2020). This finding answers Bushnell, Mansur, and Novan (2017) in part. At least for the general effectiveness of restructuring regarding fuel procurement, restructuring did not improve the level of efficiencies that the traditional regulation had established prior to restructuring.

finally, how "disadvantaged" a plant's pre-divestiture contract is relative to contracts of similar plants.

First, we build a simple agency model to derive testable hypotheses of how divestiture affects a firm's incentives through each of the three channels. We incorporate the institutional details of the U.S. electricity generation sector and specify a utility's payoff to depend not only on its negotiation effort but also on the rate of fuel costs the utility can pass on to consumers multiplied by its buyer size, the quantity of coal the utility purchases. Then, we derive that the expected change in the price paid for coal from divestiture is decreasing in the degree of pre-divestiture incentive and increasing in the degree of contract disadvantage. The model predicts that the expected price change is ambiguous with respect to size. Nevertheless, we derive a sufficient condition for the effect to be increasing in size, argue that the condition is likely to be true in practice, and verify our intuition using empirical data.

In testing the model's theoretical predictions with data, we leverage the quasi-experimental variation that a large number of states terminated their restructuring legislation around 2000 after the electricity crisis in California. Because all states considered the divestiture option as a part of electricity sector restructuring by 1998, non-divested plants can serve as a plausible control group at least conditional on observable characteristics. Specifically, we adopt a matched difference-in-differences estimator (Cicala, 2015; Han et al., 2020) in which power plants that are divested by regulated utilities are compared to similar plants that remain under the status-quo regulation. We then investigate the heterogeneous effects of plant divestitures by pre-divestiture incentives, buyer size, and contract disadvantage.

We find that firms that did not face any cost-saving incentives prior to divestiture reduced their costs by about 11%, while firms exposed to incentive regulation did not reduce their costs. This suggests that divestiture did indeed reduce the cost distortions in fuel procurement for firms that were not residual claimants on fuel cost fluctuations and that incentive regulations in the earlier periods were effective. Using size to proxy for bargaining leverage, we then find that divestiture allowed plants to better exploit their bargaining advantage. In particular, larger divested plants could reduce their procurement costs by 8%-10% relative to comparable non-divested plants. These findings are consistent with the theory of countervailing bargaining power, which predicts that bigger buyers possess greater bargaining leverage and thereby extract larger discounts from their suppliers (Stole and Zwiebel, 1996; Chipty and Snyder, 1999).[3] Finally, plants that were stuck with unfavorable pre-divestiture contracts were able to negotiate better terms than plants that paid lower prices, relative to comparable plants in the control group. Plants paying a coal-price premium of one standard deviation relative to their neighbors reduced costs by 10%-13%, suggesting that divestiture lowered the opportunity costs of contract renegotiation.

We conclude that the restructuring of the U.S. generation sector has been quite effective at reducing procurement costs for certain types of plants in the short run despite an earlier finding that it had a limited impact on the average plant (Han et al., 2020). In particular, divestiture has achieved significant cost savings for the firms with strong incentives to renegotiate existing contracts or with relatively more bargaining leverage over coal mines.[4]

[3]Our model provides a new insight to the theory of the countervailing bargaining power in the context of deregulation. In our model, while a firm's size can help the firm negotiate better prices with fuel suppliers, it also incentivizes the firm to exert more effort prior to divestiture. Because a sizable firm could have secured a favorable contract pre-divestiture, buyer size does not necessarily lead to cost savings post-divestiture.

[4]The heterogeneous treatment effect results also echo recent findings in the health economics literature showing that an increase in the relative bargaining leverage of downstream firms in buyer-sellers networks can lower negotiated input prices (see, for instance, Grennan, 2013; Gowrisankaran, Nevo, and Town, 2015; Ho and Lee, 2017)

Our empirical findings contribute to the long-standing literature on the restructuring of the U.S. electricity industry by explaining why—not just whether—certain reforms are effective. Previous studies have documented various plant-level efficiency gains from divestiture: productivity gains (Fabrizio, Rose, and Wolfram, 2007), more efficient investments (Fowlie, 2010), more efficient operations (Davis and Wolfram, 2012), and lower fuel procurement costs (Cicala, 2015; Chan, Fell, Lange, and Li, 2017; Jha, 2019).[5] In this paper, we have further identified how different firms respond differentially to the incentives created by market reforms—therefore how market efficiency would be enhanced or hampered by it— and allow regulators and policymakers to better predict which firms would respond to a proposed change in rules similar to the restructuring of the U.S. generation sector.

The rest of the paper proceeds as follows. In Section 2, we explain the background and the institutional setting in which the U.S. electricity industry restructuring took place. In Section 3, we build a theoretical model and establish main hypotheses about the operating channels of restructuring. In Section 4, we test the hypotheses using empirical data and report the estimation results. We then conclude in Section 5.

2 Background

2.1 Regulation and Deregulation of the U.S. Electricity Industry

The U.S. electricity industry was historically dominated by vertically-integrated monopolists that owned and operated the generation, transmission, and distribution of electricity in separate local markets. In exchange for the monopolist protection, these utilities were subject to a form of price regulation known as cost-of-service regulation (also referred to as rate-of-return regulation). Under cost-of-service regulation, the state public utility commission authorizes electricity prices that provide a "fair" rate of return on a utility's invested capital and adequately cover a utility's incurred operating costs.

Although cost-of-service regulation, in theory, provided regulators the ability to disallow imprudently incurred expenses, in practice, costs were mostly passed on to consumers through higher electricity prices (Joskow and Schmalensee, 1986; Abito, 2020). Because of concerns about high fuel costs and electricity prices, a handful of states experimented with alternative regulatory programs to improve technical efficiency and decrease fuel procurement costs (Sappington, Pfeifenberger, Hanser, and Basheda, 2001; Knittel, 2002). These programs provided cost-saving incentives by making utilities the residual claimant on such savings (Laffont and Tirole, 1993). Using data on coal plants from 1981 to 1996, Knittel (2002) finds evidence that incentive regulation programs such as those that are directly tied to generator performance (e.g., heat rate and equivalent availability factor programs) and those that modify the pass-through of fuel costs did lead to significant efficiency and cost improvements.

While there is evidence of success with incentive regulation programs among states that decided to implement them, a more broad national effort was initiated by the federal government in the mid-1990s to make the electricity sector more competitive.[6] The effort was focused on introducing competition in

[5]For the literature studying the deregulation of the electricity market in other countries, we refer the reader to Newbery and Pollitt (1997), Wolfram (1999), Cropper, Limonov, Malik, and Singh (2011).

[6]The Energy Policy Act (EPACT) in 1992 and a series of Federal Energy Regulatory Commission (FERC) orders following the EPACT laid a legal ground for restructuring. In 1992, FERC issued Order No. 636, known as the Restructuring Rule,

the generation segment of the electricity sector by splitting generation from transmission and distribution.[7] Although actual implementation was up to states' discretion, most states were interested in the idea of market restructuring, and all have considered the prospect of restructuring by 1998. (Fabrizio, Rose, and Wolfram, 2007).

The restructuring momentum, however, dissipated quickly in the summer of 2000. When the California electricity crisis broke out, many states stopped considering restructuring their electricity markets, and a handful that had already begun the process suspended further action. Meanwhile, the states that had already finished or had made far enough progress did not revert to the old regulation (Borenstein, 2002; Griffin and Puller, 2009). In such states, regulated utilities were required to divest their generation facilities.[8] Once divested, a plant's operating costs were no longer subject to regulation as it competed in a wholesale market. Plants in the other states remained under the status quo cost-of-service regulation.

To gain insight into the effects of electricity market restructuring and of deregulation in general, several researchers have exploited this natural experiment whereby some plants underwent divestiture while others remained under cost-of-service regulation (Bushnell and Wolfram, 2005; Chan et al., 2017; Cicala, 2015; Craig and Savage, 2013; Davis and Wolfram, 2012; Douglas, 2006; Fabrizio, Rose, and Wolfram, 2007; Hausman, 2014; Wolfram, 2005). Our paper adopts a similar strategy and explores the differences in coal procurement costs between divested and non-divested plants.[9]

2.2 Coal Procurement

Fuel costs are the largest part of power plants' operating expenses, and thus it is natural to examine the effects of deregulation on these costs. In particular, researchers have focused on costs associated with coal-fired plants to evaluate the efficacy of incentive regulation (Knittel, 2002) and market restructuring (Cicala, 2015) in lowering fuel costs.

There are two drivers of fuel costs. First is a plant's (or a multi-plant utility's) efficiency in converting coal into electricity; second is the cost of procuring coal. Incentive regulation programs such as heat rate programs aim to provide incentives to improve fuel efficiency directly. Electricity market restructuring and other incentive programs such as those limiting the automatic adjustment of electricity prices based on fuel costs would affect incentives for both fuel efficiency and procurement costs. Our paper takes coal procurement costs as the outcome variable in examining the effects of restructuring on fuel costs.

Electric utilities rely on long-term contracts with a few coal suppliers to deliver coal in regular frequencies to ensure a stable supply and price (Joskow, 1987). In our sample, plants contract with about three suppliers on average and commit almost 75% of their coal demand to the largest supplier. The

which mandated open access to the transmission system and separation of electricity sales from transmission services. FERC also mandated non-discriminatory pricing and access to transmission services (Order No. 888 in 1996) and established legal grounds for non-profit organizations (Order No. 2000 in 1999) that would manage wholesale electricity markets.

[7]See Joskow (1997) and Griffin and Puller (2009) for a detailed history of the U.S. electricity market restructuring.

[8]Utilities were allowed to transfer their generation assets to unregulated affiliates. One umbrella parent company could own both the generation facilities and the regulated transmission/distribution facilities. However, the law required the generation facilities to operate independently from the parent company's transmission/distribution department in such cases.

[9]Researchers have cautioned against a potential selection bias of this estimation strategy because states with high electricity prices were more likely to deregulate (White, Joskow, and Hausman, 1996). Fortunately, the high electricity prices were mostly due to the high construction costs of nuclear plants (Chan et al., 2017; Davis and Wolfram, 2012; White, Joskow, and Hausman, 1996). Cost-of-service regulation often reflects construction costs in electricity prices as state regulators set the prices based on the amount of capital investment that electric utilities make.

terms of these contracts specify the price, quantity, and quality of the delivered coal. Factors affecting the agreed-upon terms of the contract have a direct impact on the costs of fuel procurement.

Restructuring and divestiture make the generator the residual claimant on variation in fuel cost, increasing its incentives to negotiate better terms and altering plants' bargaining position relative to coal mines. At the same time, this is also the mechanism through which incentive regulation programs can affect procurement costs. Therefore, to the extent that utilities subject to incentive regulation programs were facing strong enough incentives to minimize coal procurement costs before divestiture, we expect to see lower coal price reductions from divestiture.

We additionally focus on two factors that affect plants' bargaining position post-divestiture: buyer size and whether the pre-divestiture contract is unfavorable or "disadvantaged". The underlying premise in choosing these two factors is that divestiture opens up the possibility of renegotiating existing contracts. Buyer size affects the bargaining leverage of power plants and the expected outcome of renegotiation, while the relative favorableness of existing contracts affects the buyer's incentive to engage in contract renegotiations.

With respect to buyer size, the countervailing power literature (Stole and Zwiebel, 1996; Chipty and Snyder, 1999) suggests that larger buyers should be able to extract larger discounts from suppliers. A large buyer can extract a greater share of the surplus since if bargaining breaks down, it is more costly for the supplier to find alternative buyers willing to take a large quantity. Under vertical contracting, larger buyers impose large externalities to smaller buyers, making the former more valuable to upstream firms. Thus, larger buyers are considered pivotal and are likely to receive better offers from upstream firms (Whinston, 2008). A utility's capacity is relevant for the coal buyers when the suppliers typically invest in large relationship-specific assets in anticipation of the deal, leading to supplier hold-up (Joskow, 1987, 1988). In this paper, we incorporate the concept of buyer size into an agency model and derive a condition under which a utility can leverage its size to obtain a better deal when a utility, in theory, can instead choose to exert more effort in contract negotiation.

The renegotiation of long-term contracts, nevertheless, involves significant sunk costs. For example, contract negotiation can incur both legal costs and compensation that buyers need to pay to sellers for future revenue loss. But, the cost of renegotiation likely decreased following divestiture, which made it easier for utilities to renew their relationships with fuel suppliers. One channel that could explain this is that a number of mergers and acquisitions accompanied the electricity market deregulation. Between 1997 and 2002, approximately 150 distinct mergers took place among coal-fired electricity producers. These acquisitions were often associated with a massive influx of capital, which could reduce the opportunity cost of buying back old contracts, and with a reduction in legal costs via combining legal and procurement services. Consolidation also increased the bargaining leverage of merged power producers, further raising the benefit of renegotiation. This effect was likely to be transitory, however, which is consistent with the earlier finding from our separate paper that *on average*, firms did not experience post-divestiture cost savings (Han et al., 2020).

3 Model

We model a utility's coal procurement decision to gain insight into the drivers of observed coal price changes following divestiture. Let utility i's payoff given negotiated coal price p_i be

$$U_i = s_i \times (v - \gamma_i p_i) \tag{1}$$

where s_i is the quantity of coal that the utility purchases (i.e., utility size), v is the surplus a unit of coal generates, and $1 - \gamma_i \in [0, 1]$ is the fraction of fuel costs that can be passed on to consumers through higher regulated electricity prices. The parameter γ_i would then be the fraction of costs retained by the utility, for instance, under incentive regulation.

Before arriving at the negotiated price p_i, the utility has to decide on the level of negotiation effort to exert. We assume that the distribution of p_i is decreasing in effort in the first-order stochastic sense, which implies that the expected coal price $E[p_i|e_i]$ is decreasing in effort. Exerting effort entails a cost (i.e., disutility of effort) equal to $\psi(e_i)$ where we assume $\psi(\cdot)$ is strictly increasing and convex, and normalize $\psi(0) = 0$. We also assume that $\psi(e_i)$ is sunk once the negotiated price is revealed.

Given these, the utility i chooses e to maximize expected payoff,

$$E(U_i) = s_i \times (v - \gamma_i E[p_i|e_i]) - \psi(e_i). \tag{2}$$

The marginal benefit of exerting effort is equal to

$$s_i \gamma_i \left| \frac{dE[p_i|e_i]}{de} \right|$$

while its marginal cost is $\psi'(e_i)$. We first see that a utility facing full cost pass-through (i.e., $\gamma_i = 0$) will choose to exert zero effort. Moreover, the marginal benefit of effort is increasing in both size (i.e., s_i) and fuel cost incentives (i.e., cost retain rate γ_i), and therefore optimal effort and expected price are, respectively, increasing and decreasing for both "larger" utilities and utilities facing higher pre-divestiture incentives.

3.1 Post-divestiture

Upon divestiture, a utility fully absorbs the costs of fuel such that the cost-retain rate, γ_{0i}, becomes 1 (i.e., $1 - \gamma_{0i} = 0$). Denote p_{0i} as the pre-divestiture coal price. Assume that by paying a sunk fixed cost of $\phi_i > 0$, a utility can renegotiate its contract and potentially decrease its coal price from p_{0i} to a new renegotiated price. We assume ϕ_i is a draw from a distribution with cumulative distribution function G and is independent of γ_{0i}, s_i and p_{0i}. A utility optimally chooses to renegotiate as long as

$$s_i \times (v - p_{0i}) < s_i \times (v - E[p_i|e_i^*]) - \psi(e_i^*) - \phi$$

where e_i^* is the optimal post-divestiture level of effort.

We now explore how the probability of renegotiation is affected by pre-divestiture incentives γ_{0i}, buyer size s_i, and pre-divestiture contract disadvantage D_i, defined as

$$D_i = p_{0i} - E[p_{0i}|e^*_{0i}]$$

where e^*_{0i} is the optimal pre-divestiture level of effort. These results will be useful in deriving our main result in the next subsection.

The probability of renegotiation after divestiture is given by

$$\mu(\gamma_{0i}, s_i, D_i) = G(\hat{\phi}_i)$$

where

$$\hat{\phi}_i = s_i \cdot (D_i + E[p_{0i}|e^*_{0i}] - E[p_i|e^*_i]) - \psi(e^*_i).$$

Consider first γ_0. The partial derivative of μ with respect to γ_0 is

$$\frac{\partial \mu}{\partial \gamma_0} = -\frac{dG}{d\phi} \cdot \left(s_i \cdot \left| \frac{dE[p_{0i}|e^*_{0i}]}{de} \right| \cdot \frac{\partial e^*_0}{\partial \gamma_0} \right). \tag{3}$$

Since the expected negotiated price is decreasing in effort and effort is increasing in γ, the probability of renegotiation is decreasing in pre-divestiture incentives. When pre-divestiture incentives are strong (i.e., high γ_0), the utility would have exerted greater effort even before divestiture, making it more likely to have a lower pre-divestiture coal price. This lower coal price reduces the attractiveness of renegotiating the contract after divestiture.

Next, we turn to the effect of size on the probability of renegotiation. Assuming the optimal post-divestiture level of effort is strictly positive—which ensures that the first-order condition for an interior solution holds—, the partial derivative of μ with respect to s_i can be written as

$$\frac{\partial \mu}{\partial s} = \frac{dG}{d\phi} \cdot \left\{ (p_{0i} - E[p_i|e^*_i]) - s_i \cdot \left| \frac{dE[p_{0i}|e^*_{0i}]}{de} \right| \cdot \frac{\partial e^*_0}{\partial s} \right\} \tag{4}$$

If we assume that the utility's outside option during renegotiation is p_{0i}, then $p_{0i} - E[p_i|e^*_i] \geq 0$. Thus, the sign of the partial derivative is ambiguous. There are two channels upon which size affects the probability of renegotiation. First, size directly magnifies the expected "reward" from renegotiation, $p_{0i} - E[p_i|e^*_i]$, hence making it more attractive for larger utilities to renegotiate. The second channel works through the optimal pre-divestiture effort. Because size increases effort, larger utilities may already have signed a favorable contract in the years before divestiture, making post-divestiture renegotiation less attractive. The overall effect of size on the probability of renegotiation depends on the relative magnitudes of these channels.

Finally, the partial derivative of μ_i with respect to D_i is simply

$$\frac{\partial \mu}{\partial D} = \frac{dG}{d\phi} \cdot s_i \tag{5}$$

which is strictly positive. Therefore, the higher the probability of renegotiation is, the more disadvantaged the pre-divestiture contract is.

3.2 Comparative Statics

Our object of interest is the change in coal prices caused by divestiture. If the utility does not renegotiate its contract after divestiture, then the price change is equal to zero. On the other hand, if the utility renegotiates its contract, then the price change is equal to $p_{0i} - p_i$ where p_i is the ex-post renegotiated price.

In the empirical section of the paper, we estimate the expectation of the coal price change conditional on pre-divestiture incentives γ_{0i}, buyer size s_i and pre-divestiture contract disadvantage D_i. In our model, this conditional expectation is

$$\Delta(\gamma_{0i}, s_i, D_i) = \mu_i \times (D_i + E[p_{0i}|e^*_{0i}] - E[p_i|e^*_i]) \tag{6}$$

where $\mu_i = \mu(\gamma_{0i}, s_i, D_i)$, the probability of renegotiation.

We perform comparative statics on $\Delta_i = \Delta(\gamma_{0i}, s_i, D_i)$ to generate the predictions of our model. We then empirically test these predictions in the next section. The following proposition summarizes the predictions of our model.

Proposition 1. *The expected price change due to divestiture is decreasing in the degree of pre-divestiture incentive and increasing in the degree of contract disadvantage. A sufficient condition for the expected price change to be increasing in size is*

$$\frac{s_i}{\mu_i}\frac{\partial \mu}{\partial s} \geq 1 - \frac{\left|\frac{dE[p_i|e^*_i]}{de}\right| \frac{\partial e^*_i}{\partial s}}{\left|\frac{dE[p_{0i}|e^*_{0i}]}{de}\right| \frac{\partial e^*_0}{\partial s}}. \tag{7}$$

Proof. The comparative statics with respect to γ_{0i} and D_i follows directly from the partial derivatives of Δ_i with respect to these variables and the corresponding comparative statics for the probability of renegotiation:

$$\frac{\partial \Delta}{\partial \gamma_0} = -\left|\frac{\partial \mu}{\partial \gamma_0}\right| \times (p_{0i} - E[p_i|e^*_i]) - \mu_i \times \left|\frac{dE[p_{0i}|e^*_{0i}]}{de}\right| \cdot \frac{\partial e^*_{0i}}{\partial \gamma_0} < 0$$

and

$$\frac{\partial \Delta}{\partial D} = \frac{\partial \mu}{\partial D} \times (p_{0i} - E[p_i|e^*_i]) + \mu_i > 0.$$

To derive the sufficient condition for an increasing effect of size on the expected price change, first note the partial derivative of Δ_i with respect to s_i:

$$\frac{\partial \Delta}{\partial s} = \frac{\partial \mu}{\partial s} \times (p_{0i} - E[p_i|e^*_i]) + \mu_i \times \left\{\left|\frac{dE[p_i|e^*_i]}{de}\right| \cdot \frac{\partial e^*_i}{\partial s} - \left|\frac{dE[p_{0i}|e^*_{0i}]}{de}\right| \cdot \frac{\partial e^*_{0i}}{\partial s}\right\}.$$

Observe that from Equation (4),

$$\frac{\partial \mu}{\partial s} > 0 \Longleftrightarrow p_{0i} - E[p|e^*_i] > s_i \cdot \left|\frac{dE[p_{0i}|e^*_{0i}]}{de}\right| \cdot \frac{\partial e^*_0}{\partial s}$$

hence

$$\frac{\partial \mu}{\partial s} \times (p_{0i} - E[p_i|e^*_i]) > \frac{\partial \mu}{\partial s} \times s_i \cdot \left|\frac{dE[p_{0i}|e^*_{0i}]}{de}\right| \cdot \frac{\partial e^*_0}{\partial s}.$$

This implies

$$\frac{\partial \Delta}{\partial s} > \frac{\partial \mu}{\partial s} \times s_i \cdot \left|\frac{dE[p_{0i}|e_{0i}^*]}{de}\right| \cdot \frac{\partial e_0^*}{\partial s} + \mu_i \times \left\{ \left|\frac{dE[p_i|e_i^*]}{de}\right| \cdot \frac{\partial e_i^*}{\partial s} - \left|\frac{dE[p_{0i}|e_{0i}^*]}{de}\right| \cdot \frac{\partial e_{0i}^*}{\partial s} \right\}.$$

Thus, it suffices that

$$\frac{\partial \mu}{\partial s} \times s_i \cdot \left|\frac{dE[p_{0i}|e_{0i}^*]}{de}\right| \cdot \frac{\partial e_0^*}{\partial s} \geq \mu_i \times \left\{ \left|\frac{dE[p_{0i}|e_{0i}^*]}{de}\right| \cdot \frac{\partial e_{0i}^*}{\partial s} - \left|\frac{dE[p_i|e_i^*]}{de}\right| \cdot \frac{\partial e_i^*}{\partial s} \right\}$$

in order for $\frac{\partial \Delta}{\partial s} > 0$. Rewriting this condition proves the proposition:

$$\frac{s_i}{\mu_i}\frac{\partial \mu}{\partial s} \geq 1 - \frac{\left|\frac{dE[p_i|e_i^*]}{de}\right| \frac{\partial e_{0i}^*}{\partial s}}{\left|\frac{dE[p_{0i}|e_{0i}^*]}{de}\right| \frac{\partial e_0^*}{\partial s}}.$$

□

Our model predicts that the expected price change due to divestiture is decreasing in pre-divestiture incentives and increasing in pre-divestiture contract disadvantage. With respect to size, the effect on expected price change is ambiguous. Nevertheless, we have derived a sufficient condition for the effect to be positive. This condition can be described as a lower bound on the elasticity of the probability of renegotiation with respect to size. This lower bound depends on the relative magnitudes of the effect of size on the expected negotiated prices before and after divestiture (i.e., $\left|\frac{dE[p_i|e_i^*]}{de}\right| \frac{\partial e_{0i}^*}{\partial s} / \left|\frac{dE[p_{0i}|e_{0i}^*]}{de}\right| \frac{\partial e_0^*}{\partial s}$). Specifically, if the effect of size is larger for higher effort (i.e., post-divestiture e_i^*) than lower effort (i.e., pre-divestiture e_{0i}^*), then the lower bound becomes negative. As long as the probability of renegotiation is increasing in size (i.e., $\frac{\partial \mu}{\partial s}$ or Equation (7) is positive), the elasticity of the probability of renegotiation with respect to size is positive (i.e., $\frac{s_i}{\mu_i}\frac{\partial \mu}{\partial s} > 0$) and greater than the negative lower bound (i.e., $1 - \left|\frac{dE[p_i|e_i^*]}{de}\right| \frac{\partial e_{0i}^*}{\partial s} / \left|\frac{dE[p_{0i}|e_{0i}^*]}{de}\right| \frac{\partial e_0^*}{\partial s}$). This is sufficient for the expected price change due to divestiture to be increasing in size (i.e., $\frac{\partial \Delta}{\partial s} > 0$).

Then, the two natural questions arise: when is the effect of size larger for higher effort, and when is the probability of renegotiation increasing in size? First, when effort and size are complements in reducing the expected price, the effect of size on the expected price is larger for larger effort. In practice, managers find it rather difficult to substitute effort and size to obtain a better offer from coal contract negotiation. This is because capacity ramp-up or -down (i.e., adjusting the operating rate of power plant generators) are often costly (Wolak, 2007). Moreover, when a power plant site is designed to host a fixed number of generators, adding or replacing a generator is infeasible or astronomically expensive. Though the true relationship between managerial effort and buyer size is latent and thus of an empirical question in nature, we posit that they are likely to be complementary. Second, the probability of renegotiation is increasing in size as long as a utility's size has not already induced the utility to obtain a favorable contract pre-divestiture. Only when the utility could expect a reasonable reward from renegotiation upon divestiture will the probability of renegotiation upon divestiture be increasing in size.[10] To summarize, when effort and size are complements and a utility's existing contract is not too favorable,

[10]More precisely, we refer the reader to the discussion following Equation (4).

then the expected price change due to divestiture is expected to be increasing in size.

4 Empirical analysis

4.1 Data

In the previous section, we have derived a core set of predictions that the impact of power plant divestiture would vary by a plant's pre-divestiture incentive, size, and contract disadvantage. We now introduce our data and explain how we link the theoretical predictions of the model to the empirical data.

We merge several survey forms collected by the Energy Information Administration (EIA) and Federal Energy Regulatory Commission (FERC) to gather information on a plant's fuel expenditure, characteristics, and divestiture status. We supplement the divestiture data with the Environmental Protection Agency's (EPA) data on operator status. For the variables investigating the operating channels of deregulation, we build a plant's size and a measure of contract disadvantage using the EIA data and rely on the Regulatory Research Associates' *Regulatory Focus* reports for information on a utility's incentive regulation. The combined data are comprehensive in surveying the fuel transactions of most fossil-fueled plants in the U.S. at the monthly level from 1990 to 2012. We direct the reader to Appendix A.1 for the specific survey forms responsible for different pieces of information.

In our theoretical model in Section 3, we have introduced a utility's pre-divestiture incentives as the fraction of fuel costs that the utility can pass on to its consumers. Empirically, the types of incentive regulation programs that encourage procurement cost savings are variants of regulation that contain fuel adjustment clauses (FACs). FACs allow for an (almost) automatic adjustment of electricity prices in response to unanticipated and significant changes in fuel prices. Historically, these regulatory practices first gained popularity during the energy crisis of the 1970s. However, with more stable fuel prices and further recognition that FACs provide rather poor incentives, several states started to move away from traditional FACs.[11] As state regulators eliminated fuel adjustment clause or replaced it with a modified fuel adjustment clause that does not fully pass through cost overruns, utilities have begun to bear a portion of fuel expenditures.

Along with the elimination or the modification of the traditional FACs, some utilities faced a price cap, a rate freeze, or an earnings sharing agreement that were also aimed to reduce the costs of fuel procurement. Price cap regulation sets a maximum price that a utility can charge over time combined with built-in adjustments for productivity improvements and inflation, while a rate freeze disallows any change in prices for a given period of time similar to a rate case moratorium. Both types of regulations, at least partially, decouple the regulated electricity price from the firm's cost, which provides an incentive for the firm to reduce fuel costs.[12] Finally, an earnings sharing program has allowed utilities to retain

[11] MO, MT, OR, TN and WA do not allow automatic fuel adjustments for coal. CT, IA, MD, NY, VA, WI and WV introduced a modified fuel adjustment clause before 1998. For example, starting in 1983, utilities in New York had to bear any fuel costs above 60% to 80% (depending on the utility) of a predetermined level. In general, automatic adjustment clauses tend to be implemented at the state level rather than at the individual utility level.

[12] MT, NY, OR, and WA implemented price cap regulation for certain utilities. LA, MO, and NY implemented a rate freeze for certain utilities during the period under consideration.

some of the gains from the reductions in operating costs such as fuel procurement.[13] We, therefore, classify a utility as subject to incentive regulation if it operates without any FAC or is under a modified FAC, a price cap, a rate freeze, or an earnings sharing agreement by 1997.

For buyer size, we use a utility's generation capacity as a proxy. In theory, the amount of coal a utility purchase does not necessarily equal the quantity of coal the utility can burn with a given capacity. However, in practice, coal-fired power plants are typically used for "baseload" generation (i.e., running at almost full capacity except for maintenance), suggesting that a utility's generation capacity is a *de facto* cap for its coal consumption. We, thus, define buyer size to be a utility's generation capacity as of 1997.

The pre-divestiture contract disadvantage in our model is defined as $p_{0i} - E[p_{0i}|e^*_{0i}]$, the difference between the realized pre-divestiture price and the expected pre-divestiture price given the level of effort a utility has exerted prior to divestiture. It is implausible in reality that utilities observe the full distribution of coal prices for any given level of effort. We instead use the average price of a utility's neighbors as the expected price. To the extent that nearby utilities exert a similar level of effort, our assumption would be innocuous. Among the 27 states in our sample, only 6 ever operated more than one different incentive program between 1990 and 1997. So, most of the local utilities within the same state were likely to face similar incentives at any given year. Nevertheless, we acknowledge that certain neighboring utilities (e.g., close-by inter-state ones) could be subject to different incentive programs. In Appendix A.2, we show that our results are robust to extending our matching criterion to include whether the plants were under the same incentive regulation conditions. So, we argue that the average coal price paid by local neighbors facing the same incentive regulation should provide a reasonable assessment of how disadvantaged one's contract is relative to the average contract that the firm could have secured for the same level of effort. To be precise, we define a firm's contract disadvantage to be the percentage difference between its own contract price of coal in 1997 and the average contract price of its neighbors in the same year.

Lastly, we note a caveat to the data that a part of the expenditure data is missing for the divested plants. The EIA did not require divested plants to report their fuel expenditure once they became divested. The agency resumed the data collection in 2002. Because most divested plants were sold off between 1999 and 2001, they have two years of missing data on average. This affects the interpretation of our analysis. A difference-in-differences estimator would measure the differences in the outcome variable before and *two-years-after* the treatment as opposed to before and after.

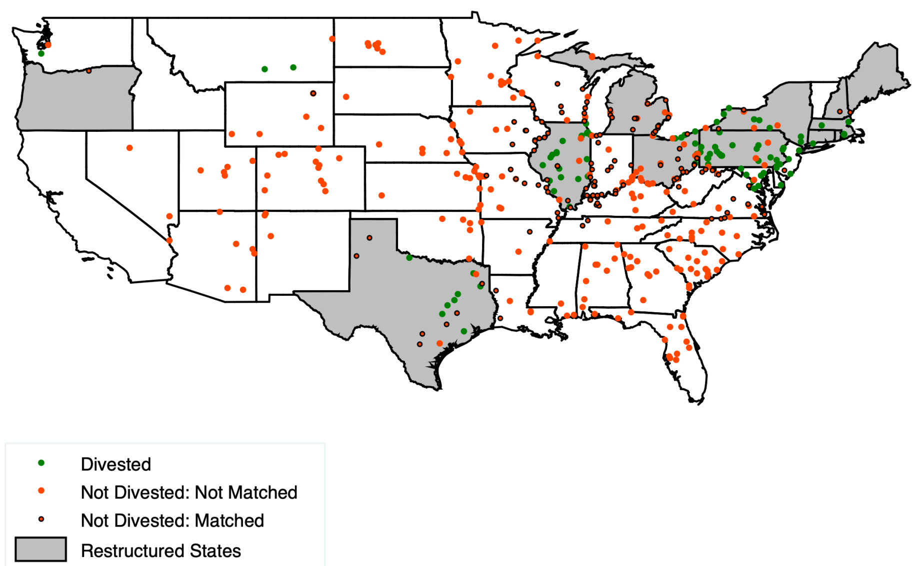
4.2 Research Design

In this paper, we employ a *matched* difference-in-differences (DiD) estimator similar to the synthetic control approach in Abadie, Diamond, and Hainmueller (2012). The treatment group consists of divested plants in restructured states, while the control group consists of regulated plants that remain under the status quo cost-of-service regulation. For the matching procedure, we closely follow Cicala (2015). We match each divested plant to (at most) 10 nearest non-divested plants located within 200 miles, burning the same coal type prior to divestiture (December 1997). Coal type can be bituminous (high sulfur), sub-bituminous (low sulfur), or other.

[13] CT, IA, IN, LA, MA, MO, MT, NY, OH, and OR implemented earnings sharing for certain utilities.

The justification for matching is evident in Figure 1, where we plot the locations of coal-fired power plants in the U.S. by their eventual divestiture status. While the divested plants are disproportionately located in the Northeast and the Midwest with the exception of a handful of plants in Texas, Montana, and Washington, shipping costs to different regions of the country have varied differentially over time (Busse and Keohane, 2007; Energy Information Administration, 2004), which could confound the treatment effect associated with divestiture. Because sub-bituminous coal is primarily produced in the Western States (e.g., Powder River Basin in Montana and Wyoming) and bituminous coal is mostly mined from the Appalachian Mountains, we also match on coal type. The matching estimator thus compares plants facing similar fuel procurement conditions over time.

Figure 1: Map of Coal-Fired Plants in the United States



Notes: Plants that exit before 1997, enter after 2002, and cease reporting after 2002 are not shown on the map.

As for the estimation, we employ a weighted least squares (WLS) at the plant-month level—not the utility-month level—and cluster standard errors at the utility level.[14] We treat the plant as our unit of analysis, which implicitly assumes that negotiation is managed with a coal supplier at the plant level. This is contrary to anecdotal evidence that multiple plants are often negotiated together by a single operating company (Han et al., 2020). We have three reasons for this choice. The first reason is practicality. Divestiture entails a change in operator, and therefore post-treatment data for buyer-operators will be missing. Second, the average and the median number of plants per operator are respectively 2.02 and 1. Any given operator is responsible for managing, at most, a couple of plants. Contract-level data from the Coal Transportation Rate Database reveal that a contract is responsible for deliveries to 1.7 plants

[14]For each treated plant in a given year-month, the number of matched control plants is given by $m_{jt} \leq 10$. The matched control plants receive weight $\frac{1}{m_{jt}}$, and the treated plants receive a weight equal to one. Therefore the weights for the matched DiD estimator sum to 2·(# of divested plants)·(# of months).

on average with a median of 1 plant. Lastly, when the econometrician does not observe the true level at which contracts are negotiated, collapsing the data to the utility level could result in losing relevant variation. A preferred remedy is to cluster standard errors at the utility level to account for a strong correlation between the residuals belonging to the same utility if the contracts are indeed negotiated at the utility level.

Because our primary goal is to understand the mechanisms through which deregulation delivers fuel cost reductions, we investigate whether the effect of deregulation on the delivered coal prices varies by a plant's incentive regulation, size, and contract disadvantage. The following is the estimating equation that we bring to the data:

$$\begin{aligned}\log(price)_{jt} =& \alpha_0 \mathbb{1}(divest)_{jt} + \alpha_1 \mathbb{1}(divest)_{jt} \cdot incentive_j + \alpha_2 \mathbb{1}(divest)_{jt} log(capacity)_j \\ &+ \alpha_3 \mathbb{1}(divest)_{jt} \cdot (disadv.)_j \cdot \mathbb{1}(disadv. > 0)_j \\ &+ \alpha_4 \mathbb{1}(divest)_{jt} \cdot (disadv.)_j \cdot \mathbb{1}(disadv. \leq 0)_j + \gamma_j + \delta_t + \epsilon_{jt}\end{aligned} \tag{8}$$

The outcome variable, $log(price)_{jt}$, is the total price of delivered coal per unit of energy (MMBtu) for plant j in year-month t. It represents the price of delivered coal at the final destination, and as such not only includes the expenditure on fuel but also the costs of transportation. $1(divest)_{jt}$ is a DiD indicator for whether plant j is divested and whether time t is after the year-month of the divestiture. We measure the variable $incentive_j$ using the number of years a plant is subject to incentive regulation between 1990 and 1997. While incentive regulation broadly include many different programs, we are particularly interested in programs that either specifically target fuel procurement costs or are closely related to it: namely, a modified or no FAC, a price cap, a rate freeze, or an earnings sharing agreement. The size or buyer power of a plant is measured by $log(capacity)_j$ as of 1997 in logged megawatts. Lastly, a plant's contract disadvantage, $(disadv.)_j$, is measured as the percentage difference between the plant's coal price under its contracts as of 1997 and the average contract price of its neighbors in the same year. That is, $(disadv.)_j$, is equal to $\frac{p^c_{j,97} - \overline{p^c_{j,97}}}{\overline{p^c_{j,97}}}$ where $p^c_{j,97}$ is the delivered coal price at plant j under its contract(s) in 1997 and $\overline{p^c_{j,97}}$ is the average contract price for plant j's neighbors in the same year.

For the contract disadvantage variable, we allow for an asymmetric response to divestiture by interacting it with the indicators $\mathbb{1}(disadv. > 0)_j$ and $\mathbb{1}(disadv. < 0)_j$. Note that if $\alpha_3 = \alpha_4 < 0$ in Equation (8), this could merely reflect mean reversion as long-term contracts—both favorable and unfavorable—gradually expire and get replaced with the average market price of coal. We are interested in if this process is different for divested plants that pay above vs. below market prices. We thus allow for the possibility that divestiture serves as an impetus to renegotiate and improve contracts for plants stuck with unfavorable contracts, but not having the same effect for plants with favorable contracts (i.e., $\alpha_3 < 0, \alpha_4 = 0$).

Despite our matching procedure to reinstate similar procurement conditions for the treatment and the control group, one may raise a reasonable worry that local utilities subject to different incentive regulation programs face different cost-saving incentives. In the Appendix, we show the robustness of our results by imposing the pre-divestiture regulatory status of each plant to the set of matching criteria, which we refer to as "additional matching". Specifically, the robustness of the validity of pre-tests is shown in Appendix A.2 and of the regression results is shown in Appendix A.4.[15]

4.3 Validity Tests, Outlier Detection, and Average Treatment Effect

To assess the validity of our research design, we first conduct two standard pre-tests of a DiD estimator: a balance-of-covariates test and a test for equal pre-trends. While the results of standard pre-testing concede no clear sign of violating the research design, we have previously documented that a group of power plants influenced by an event unrelated to treatment contributes to more than half of the average treatment effect (Han et al., 2020). We briefly discuss the presence of outliers in the data and its impact on the estimator but invite the reader to the afore-mentioned paper for the details of outlier detection.

First, we present the balance-of-covariates test in Table 1.[16] The table illustrates that divested and non-divested plants are well-balanced in the periods before divestiture except for the delivered coal prices. The fact that plants in the treated sample paid 7-12% higher coal prices in the pre-period is somewhat surprising since matching restricts the econometrician to compare plants based on coal type and geography. As explained in (Han et al., 2020), this difference is caused by an outlier contract, but at this stage, it is not necessarily inconsistent with the DiD assumption as including plant fixed effects in the regression model can easily absorb the level differences in the outcome variable between the treatment and the control group.

Next, we turn to a standard pre-trend test, which is the most commonly used empirical test to assess the validity of a DiD research design. Figure 2 Panel (a) reveals no visual evidence of differential trends between the treatment and control groups. Panel (b) shows that average prices were higher for treated plants than for control plants. In Appendix Table A.2, we formally test whether the two groups have differential linear trends before 1997. The relevant coefficients on $time_t \cdot \mathbb{1}(divest)_j$ suggest that there is a slight negative pre-trend in the full sample of -0.2 percentage points per month, but the slope coefficient is not statistically significant (t-statistic: -0.5). Thus we cannot reject the null hypothesis of equal pre-trends using the standard pre-trend test. Hence, neither the balance-of-covariates test nor the pre-trend test raises an apparent red flag in using a DiD research design.

However, the standard pre-testing should not be the only metric to validate a DiD framework. The recent applied econometrics literature has questioned the statistical power of the standard pre-tests (Roth, 2018; Freyaldenhoven, Hansen, and Shapiro, 2019). In Han et al. (2020), we apply a novel outlier detection tool from the robust statistics literature to the same setting as this paper and detect a group of outlier plants that escapes the pre-tests. Specifically, 7 out of 87 treated plants that belong to the same electric utility in Illinois, Commonwealth Edison (ComEd), do not satisfy the underlying assumption of a DiD research design. When ComEd had renegotiated its coal contracts before—and for reasons unrelated to—deregulation, we cannot find comparable non-divested plants that experienced a similar procurement and renegotiation history as ComEd. In other words, there is no counterfactual reference for the change in ComEd's costs in the absence of treatment.

[15] Taking a (divested) plant-month as the unit of observation, the average number of matched control plants used for a treatment plant is 6.46 when we match on distance and coal type only. When we include prior incentive regulation status in the matching criteria, the average number of matched control plants used for a treatment plant decreases to 4.92 (for divested plants without incentive regulation) and 3.01 (for divested plants with incentive regulation).

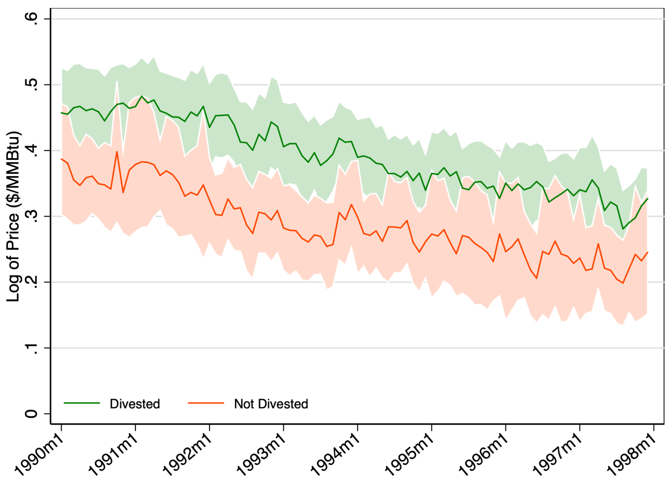
[16] Appendix Table A.1 presents a similar table but using additional matching.

Table 1: Summary Statistics for Divested and Non-Divested Plants

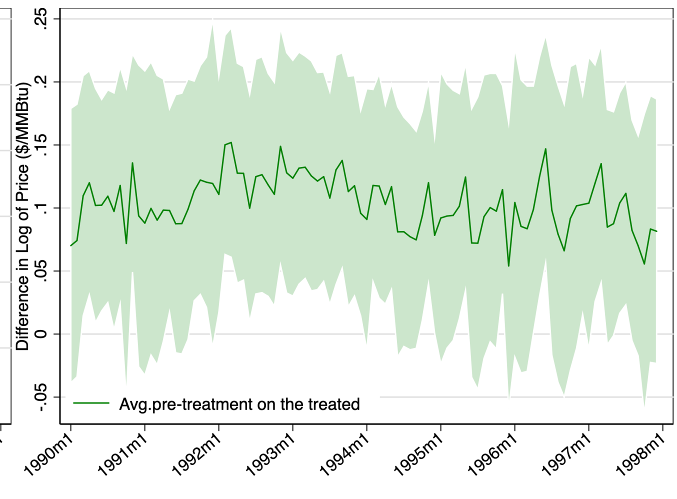
	(1)	(2)	(3)
	Divested	Not divested	Difference
Panel A: Time-invariant statistics as of 1997			
Plant vintage	1960.23	1960.15	0.08
	[11.48]	[15.27]	(2.85)
Percent scrubbers installed	0.24	0.26	-0.01
	[0.43]	[0.44]	(0.08)
Years of incentive regulation	2.22	1.96	0.26
	[3.27]	[3.29]	(0.61)
Panel B: Monthly average statistics between 1990 and 1997			
Price ($/MMBtu)	1.54	1.37	0.17***
	[0.47]	[0.31]	(0.06)
Log(price)	0.39	0.29	0.10**
	[0.29]	[0.24]	(0.04)
Millions MMBtu delivered	3.61	3.15	0.46
	[3.55]	[3.23]	(0.57)
Percent spot market	0.26	0.28	-0.02
	[0.34]	[0.37]	(0.06)
Percent in-state	0.44	0.43	0.01
	[0.46]	[0.45]	(0.08)
Percent bituminous	0.78	0.79	-0.02
	[0.41]	[0.39]	(0.07)
Percent sub-bituminous	0.15	0.13	0.02
	[0.35]	[0.32]	(0.05)
Heat content (MMBtu/ton)	23.07	22.81	0.25
	[3.99]	[3.99]	(0.73)
Sulfur content (lbs/MMBtu)	1.42	1.60	-0.18
	[0.83]	[0.96]	(0.14)
Ash content (lbs/MMBtu)	9.81	10.05	-0.24
	[5.87]	[6.89]	(1.08)
Distance to mine (mi.)	278.21	244.56	33.64
	[304.39]	[273.55]	(44.38)
Annual capacity (MW)	973.79	867.13	106.65
	[741.41]	[702.32]	(130.92)
Annual capacity factor	0.50	0.49	0.01
	[0.19]	[0.21]	(0.04)
Plants	87	119	207

Notes: Panel A contains time-invariant statistics as of 1997. Panel B contains monthly averages between 1990 and 1997. Matching is based on distance and coal type in 1997. Maximum nearest neighbors is 10. The sample period is January 1990-December 1997. Standard deviations are in brackets for Columns (1) and (2). Standard errors are clustered at the plant level in parentheses for Column (3). Asterisks denote * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Figure 2: Pre-Trends in Delivered Coal Prices



(a) Pre-Trends by Treatment Status



(b) Price Difference

Notes: Panel (a) shows the weighted average of log delivered coal costs in $/MMBtu for divested and non-divested plants. Panel (b) shows the difference in the weighted average of log delivered coal costs between divested and non-divested plants. In both panels, non-divested plants receive a weight $\frac{1}{m_j}$ where m_j is the number of non-divested plants matched to a divested plant j. This weighting structure is equivalent to creating a synthetic control for each divested plant. Matching is based on distance and coal type in 1997. Maximum nearest neighbors is 10. Shaded regions indicate 95% confidence intervals based on the standard errors from a regression of log cost on year-month fixed effects for treatment vs. control plants ($\log(cost)_{jt} = \gamma_t + \gamma_t \cdot 1(Divest)_j + \epsilon_{jt}$) clustered at the plant level.

While there are several options to account for the presence of the outliers, our preferred specification is to simply drop them from the sample. Once we drop ComEd, we obtain the average treatment effect to be about 6% and statistically insignificant. However, the estimate is not sensitive to a particular choice of how we account for the outlier. The average treatment effect is 12% and statistically significant when the outliers are not accounted for and left intact in the sample. For the rest of the paper, we maintain a sample excluding the seven ComEd plants. We refer the reader to Han et al. (2020) for the details of ComEd's unique contract renegotiation history and different ways that we have accounted for the outliers.

4.4 Heterogeneous Treatment Effect Estimates

We now present the results of estimating our regression model. In Table 2 Column (1), we present the regression estimates of Equation (8). We also estimate specifications where we treat each mechanism individually in Columns (2) to (4). All regressions use the sample without the ComEd outliers. We prefer the estimates from the joint model in Column (1) but also discuss the separate models in Columns (2) to (4).

For each additional year of incentive regulation, we find that the effect of divestiture is reduced in magnitude by 1.5 and 2.1 percentage points in Columns (1) and (2), respectively. On the one hand, Column (2) suggests that the divested plants that were never under incentive regulation (i.e., zero years of incentive regulation between 1990 and 1997) achieved about 11.4% cost reductions. On the other hand, the estimates in Column (2) imply that firms that faced incentive regulation for six or more years dur-

Table 2: Heterogeneous Effects of Divestiture on Log(Price)

	(1)	(2)	(3)	(4)
$\mathbb{1}$(Divest)	$0.452^{***}_{\dagger\dagger\dagger}$	$-0.114^{***}_{\dagger\dagger}$	$0.598^{***}_{\dagger\dagger\dagger}$	-0.011
	(0.121)	(0.042)	(0.134)	(0.043)
	[0.118]	[0.048]	[0.127]	[0.049]
$\mathbb{1}$(Divest)·(Incentive years)	$0.015^{***}_{\dagger\dagger}$	$0.021^{***}_{\dagger\dagger\dagger}$		
	(0.005)	(0.005)		
	[0.007]	[0.006]		
$\mathbb{1}$(Divest)·log(Capacity)	$-0.079^{***}_{\dagger\dagger\dagger}$		$-0.100^{***}_{\dagger\dagger\dagger}$	
	(0.016)		(0.020)	
	[0.014]		[0.019]	
$\mathbb{1}$(Divest)·Disadv.·$\mathbb{1}$(Disadv.>0)	$-0.388^{***}_{\dagger\dagger\dagger}$			$-0.529^{***}_{\dagger\dagger\dagger}$
	(0.096)			(0.103)
	[0.099]			[0.114]
$\mathbb{1}$(Divest)·Disadv.·$\mathbb{1}$(Disadv.<0)	-0.025			0.159
	(0.232)			(0.244)
	[0.241]			[0.249]
Avg. # of Matched Neighbors	6.3	6.3	6.3	6.3
Year-Month FE	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes
Plant-Level Clusters	184	184	184	184
Utility-Level Clusters	93	93	93	93
Divested Plants	80	80	80	80
Control Plants	104	104	104	104
R^2	0.791	0.782	0.783	0.784
Observations	34,145	34,145	34,145	34,145

Notes: Dependent variable is the logarithm of the coal price. Capacity is at the plant level. Matching is based on distance and coal type in 1997. Maximum nearest neighbors is 10. Average number of matched neighbors is 6.3. Standard errors are clustered at the plant level in parentheses and at the utility level in square brackets. A plant's parent utility is determined based on plant ownership as of 1997. Asterisks denote statistical significance at the plant level clustering * p<0.10,** p<0.05, *** p<0.01. Daggers denote statistical significance at the utility level clustering † p<0.10,†† p<0.05, ††† p<0.01.

ing the pre-period did not reduce their costs at all. In Table A.4 in the Appendix, we decompose the treatment effect by an indicator variable for whether plants were ever subjective to incentive regulation before divestiture. The treatment effect for the divested plants ever subject to incentive regulation was 11.2-13.7 percentage points higher that that for the divested plants never subject to incentive regulation.[17] Our findings illustrate that cost reduction from divestiture depended strongly on the prior regulatory regime and that new market pressures provided limited incentives for the plants that were already exposed to incentive regulation.

Next, we find that as a plant's capacity doubles, costs decrease by 7.9% and 10.0% in the joint and separate models (i.e., Columns (1) and (3)), respectively. In our theoretical model, we have established that the divestiture effect is larger for larger plants only when size and effort are complements in reducing the price paid for coal. We, then, conjectured that they are less likely to be substitutes in reality because capacity adjustments are often costly, if not infeasible. The empirical data support that the response to divestiture was indeed highly concentrated among larger plants. In Appendix Table A.4, we decompose the treatment effect by plants with above-median capacity and those below the median. We find that divested plants with a generation capacity larger than a median plant reduced fuel costs by about 12.0%-14.0% while the divested plants with below-median capacity made no cost savings. These results support our hypothesis that effort and size are likely complementary in eliciting a better price offer from negotiating a contract.

Finally, we find that plants tied to unfavorable pre-divestiture contracts were able to negotiate better terms upon divestiture than comparable plants in the control group. The effect is highly asymmetric. Plants that were paying a premium of one standard deviation (25%) relative to their neighbors reduced costs by 9.7% and 13.2% in the joint and separate models, respectively. But, divested plants with favorable contracts did not experience a change in their fuel costs post-deregulation. The last row of the estimates in Table 2 suggests that their cost even increased, although the coefficients are imprecisely estimated to draw any meaningful conclusions. In Appendix Table A.4, we decompose the treatment effect simply by whether the contract price is unfavorable (i.e., $\mathbb{1}(disadv. > 0)$) without further interacting the indicator variable with the linear disadvantage term. We find that divested plants paying above the average price of the neighbors achieved about 11.5%-12.0% cost reductions while the divested plants paying below the average had small and statistically insignificant 2.2% cost reductions. Therefore, we conclude that divestiture allowed plants to renegotiate "bad" contracts, but plants with favorable contracts upheld the existing accord. This result hints at a mechanism through which divestiture delivers a reduction in fuel expenditure. A change in the ownership structure driven by market restructuring created a natural excuse for the plants to review the value of existing contracts to which they had been tied unwillingly or indifferently (thanks to FACs) for years. Divested firms could leverage newly injected capital to buy out the old contracts. The opportunity costs of contract renegotiation—for example, facing a lawsuit for breaching a contract's duration[18]—were also lowered when firms could combine legal services for divestiture and contract renegotiation.

[17] Since about 50% of divested plants were never subject to incentive regulation while the other half faced incentive regulation for about five years on average, we can deduce that the overall average treatment effect is about 6.5% (i.e., $((-0.114) + (-0.114 \cdot 0.021 \cdot 5))/2$) though we find this estimate to be statistically insignificant (Han et al., 2020).

[18] See Han et al. (2020) for the examples such lawsuits between ComEd and its coal suppliers.

5 Conclusion

Our theoretical model and empirical results illustrate that the success of electricity market restructuring hinges on the cost-minimization incentives utilities face. Utilities that were the residual claimants on fuel cost variation prior to divestiture did not achieve a reduction in procurement cost, confirming that pre-divestiture incentive regulations were effective at eliminating the moral hazard problem associated with fuel cost negotiation. Similarly, plants that had in theory the most to gain from renegotiating contracts did indeed benefit from divestiture. Firms that got divested with highly unfavorable contracts were able to force the renegotiation of old contracts down to "market prices", and only the divested plants with above-median capacity were able to (re)write new contracts in their favor.

In all three cases, these results lead us to conclude that divestiture allowed the new owners to more easily "break" the existing contracts. Divestiture improved the bargaining position of power plants relative to coal mines. As divestiture-associated M&A activities sprang an injection of capital into the electricity industry, an option to source coal from different suppliers became a credible threat. New capital could surely accommodate potential compensation to a coal supplier in case existing contracts get terminated early. An alternative channel is that divestiture might have lowered the cost of renegotiating existing long-term contracts, for example, by increasing the legal possibilities to argue a renegotiation in court.

These unobserved costs, such as the legal costs for disputes in court, bring us to a caveat in interpreting the results that some of the estimated cost savings from divestiture are offset by transfers. When divested plants renegotiate ("buy out") not-yet-expired contracts, the price of delivered coal will go down but sometimes in exchange for an unobserved financial commitment (e.g., a lump-sum payment or a future purchase commitment).[19] In addition, the estimates do not reflect legal costs. Therefore, while some of the estimated cost reductions reflect an improved (incentive to exploit) bargaining power, part of these gains could have been transferred back to coal mines.[20] In that case, the estimated average treatment effect overstates the true cost savings from divestiture. Over time, however, as contracts expire, one might reasonably expect that divested plants will follow their incentives to negotiate harder and achieve true cost savings.

Even so, the incentives the power plants faced upon divestiture varied substantially. When firms' incentives can differ significantly within the same local market, fuel cost savings from deregulation would remain an empirical question for each institution—different segments in the same industry, needlessly to mention different coal plants in different countries. For example, we expect our findings do not generalize to the plants burning natural gas as a primary fuel source in the U.S. When gas-fired power plants rely mostly on spot market purchases for fuel deliveries rather than bilateral contracts, divestiture would play a limited role in shifting the bargaining power in favor of the plants relative to the fuel suppliers. That is, of course, until future research sheds new light.

[19] For example in Han et al. (2020), we document that the divestiture of ComEd plants involved a lump-sum settlement payment to the new buyer for the high-cost coal contracts that the old buyer signed.

[20] The fact that new owners were willing to buy back bad contracts suggests that post-deregulation mergers between utilities led to an increase in profits. However, it is not clear if this added profitability originated from better "market" incentives to negotiate lower rates (a reduction in moral hazard), increased buyer power, or actual market power in electricity markets.

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A Appendix – For Online Publication Only

A.1 Data

The Energy Information Administration (Survey forms EIA-423, *Monthly Cost and Quality of Fuels for Electric Plants Report* and EIA-923, *Power Plant Operations Report*) and Federal Energy Regulatory Commission (Survey form FERC-423, *Monthly Report of Cost and Quality of Fuels for Electric Plants*) provides monthly data on fuel receipts and costs from power plants. Shipment level information on received fuel quantities, costs and qualities (heat or Btu content, sulfur content, ash content) as well as supplier information (supplier name, shipment origin and mine type) are reported by any power plant with the nameplate capacity above 50MW. The fuel cost of divested plants are confidential and the EIA grants access for academic research upon signing a data non-disclosure agreement.

Data on divestitures are compiled from four sources. First, EIA's *Electric Power Monthly* reports plant divestitures in tables titled "Electric Utility Plants Sold/Transferred and Re-classified as Non-utility Plants." Second, EIA survey form 906 *Power Plant Report* records a change in regulatory status of power plants. Third, the Environmental Protection Agency's (EPA) *Emissions and Generation Resource Integrated Database (eGRID)* provides information on dates on which power plants experience a change in their operators. Lastly, we supplement plant divestiture data with the proprietary power plant MA data from Thomson Reuters. Divestiture dates can be inferred from the changes in plant operators. While EIA-906, *eGRID* and the MA data do not identify additional power plants as divested facilities in addition to the EIA's *Electric Power Monthly* reports, the databases provide additional information on the exact timing of divestitures.

Data on plant characteristics such as generating capacity, facility location, yearly generation, public ownership, and SO_2 desulfurization equipment come from various survey forms. EIA-860 *Annual Electric Generator Report* (superseded EIA-876) provides the capacity data. eGrid and EIA-860 provide the location data. EIA-923 (previously EIA-759/906/920) provide the generation data. EIA-860, EIA-861 *Annual Electric Power Industry Report* and EIA-906 provide the public ownership data. EIA-767, *Annual Steam-electric Plant Operation and Design Data*, EPA's *Air Markets Program Data* and EIA-860 report the abatement equipment data.

Finally, the *Regulatory Focus* reports prepared by Regulatory Research Associates (RRA) provide data on incentive regulation. These proprietary reports provide detailed annual information on any form of incentive regulation that utilities in each state are subject to, and all the adjustment clauses currently being implemented by the state regulator. RRA publishes these reports for each year and for each state.

A.2 Validity Tests Using A Different Matching Criterion

Table A.1 shows the summary statistics when we include pre-divestiture regulatory status in the set of matching criteria. As discussed in Section 2, incentive regulation programs provide cost-saving incentives by making utilities the residual claimant on fuel cost variation, but the fraction of plants under incentive contracts differs between the treatment and the control groups. This difference can affect the treatment effect estimate, since the type of incentive contracts facing non-divested control plants should affect how firms react to aggregate fluctuations in fuel price; potentially violating the common trends assumption. Matching on incentive regulation status helps to mitigate this concern.

Panel (a) shows that the difference in coal prices is mitigated relative to the standard matching in Table 1 once we match on the type of incentive regulation. While additional matching comes at a cost of losing 6 divested and 32 non-divested plants, both standard and additional matching approaches yield similar results despite the fact that different matching criteria assign different weights to the matched control plants. That is, the rest of the plant characteristics is balanced between divested and non-divested plants with the additional matching criteria.

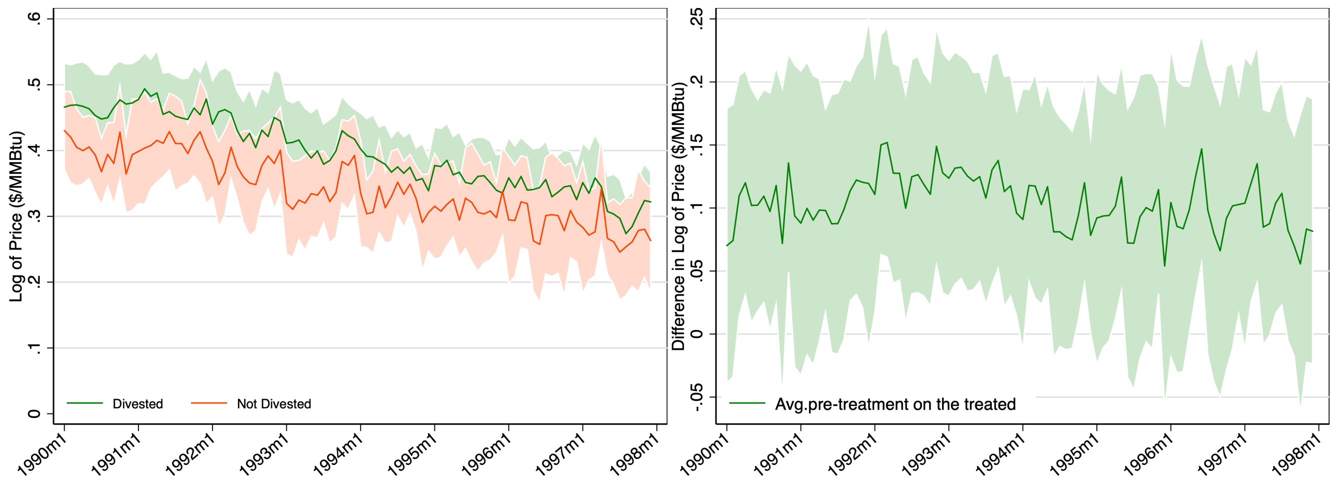
Next, we turn to a pre-trend test with additional matching. Figure A.1 Panel (a) reveals no visual evidence of differential trends between the treatment and control groups. Panel (b) shows that average prices for treated plants were higher than for control plants. In Appendix Table A.3, we formally test whether the two groups matched based the additional criteria have differential linear trends before 1997. The relevant coefficients on $time_t * \mathbb{1}(divest)_j$ suggest that there is a slight negative pre-trend of -0.3 percentage points per month, but the slope coefficient is not statistically significant (t-statistic: -0.6). Thus we cannot reject the null hypothesis of equal pre-trends using the standard pre-trend test. Hence, regardless of the choice of matching, neither the balance-of-covariates test nor the pre-trend test suggests a problem with the DiD estimator.

Table A.1: Summary Statistics for Divested and Non-Divested Plants With Additional Matching

	(1)	(2)	(3)
	Divested	Not divested	Difference
Panel A: Time-invariant statistics as of 1997			
Plant vintage	1960.19	1959.44	0.75
	[11.70]	[15.35]	(3.42)
Percent scrubbers installed	0.25	0.20	0.05
	[0.43]	[0.40]	(0.08)
Percent incentive regulated	0.47	0.47	-0.00
	[0.50]	[0.50]	(0.12)
Years of incentive regulation	1.95	3.32	-1.37
	[3.12]	[3.70]	(0.93)
Panel B: Monthly average statistics between 1990 and 1997			
Price ($/MMBtu)	1.54	1.44	0.11*
	[0.46]	[0.28]	(0.06)
Log(price)	0.40	0.34	0.05
	[0.27]	[0.20]	(0.04)
Millions MMBtu delivered	3.70	2.83	0.86
	[3.52]	[2.94]	(0.59)
Percent spot market	0.27	0.22	0.05
	[0.34]	[0.32]	(0.04)
Percent in-state	0.46	0.42	0.04
	[0.46]	[0.46]	(0.10)
Percent bituminous	0.78	0.81	-0.03
	[0.41]	[0.37]	(0.08)
Percent sub-bituminous	0.14	0.11	0.03
	[0.34]	[0.28]	(0.05)
Heat content (MMBtu/ton)	22.95	22.97	-0.02
	[3.99]	[4.09]	(0.97)
Sulfur content (lbs/MMBtu)	1.49	1.47	0.02
	[0.83]	[0.97]	(0.17)
Ash content (lbs/MMBtu)	10.07	9.94	0.13
	[6.09]	[7.63]	(1.39)
Distance to mine (mi.)	272.52	273.99	-1.47
	[314.75]	[260.05]	(47.47)
Annual capacity (MW)	1001.04	857.75	143.29
	[751.08]	[729.31]	(169.69)
Annual capacity factor	0.50	0.46	0.04
	[0.19]	[0.20]	(0.05)
Plants	81	93	174

Notes: Panel A reports time-invariant averages in 1997. Panel B contains monthly averages between 1990 and 1997. Matching is based on distance, coal type in 1997 and pre-divestiture regulatory status of each plant. Maximum nearest neighbors is 10. The sample period is January 1990-December 1997. Standard deviations are in brackets for Columns (1) and (2). Standard errors are clustered at the plant level in parentheses for Column (3). Asterisks denote * p<0.10, ** p<0.05, *** p<0.01.

Figure A.1: Pre-Trends in Delivered Coal Prices With Additional Matching



(a) Pre-Trends by Treatment Status

(b) Price Difference

Notes: Panel (a) shows the weighted average of log delivered coal costs in \$/MMBtu for divested and non-divested plants. Panel (b) shows the difference in the weighted average of log delivered coal costs between divested and non-divested plants. In both panels, non-divested plants receive a weight $\frac{1}{m_j}$ where m_j is the number of non-divested plants matched to a divested plant j. This weighting structure is equivalent to creating a synthetic control for each divested plant. Matching is based on distance, coal type in 1997 and pre-divestiture regulatory status of each plant. Maximum nearest neighbors is 10. Shaded regions indicate 95% confidence intervals based on the standard errors from a regression of log cost on year-month fixed effects for treatment vs. control plants ($\log(cost)_{jt} = \gamma_t + \gamma_t \cdot 1(Divest)_j + \epsilon_{jt}$) clustered at the plant level.

A.3 A Pre-trend Test Using A Regression Approach

In Table A.2, we test for equal linear pre-trends using a regression, as discussed in the main text in Section 4.3. The coefficient on $Time \cdot \mathbb{1}(Divest)$ report a differential trend for divested plants relative to non-divested plants. Column (1) uses the sample with the standard matching while Column (2) uses the sample with the additional matching. The statistically insignificant coefficients on $Time \cdot \mathbb{1}(Divest)$ in both of the columns suggest that the delivered coal prices of divested plants were trending differentially from those of non-divested plants before divestiture. One cannot reject equal pre-trends.

Table A.2: Hypothesis Tests on Linear Pre-Trends

	(1)	(2)
Time	$-0.0016^{***}_{\dagger\dagger\dagger}$	$-0.0016^{***}_{\dagger\dagger\dagger}$
	(0.0002)	(0.0002)
	[0.0003]	[0.0003]
Time·$\mathbb{1}$(Divest)	-0.0002	-0.0003
	(0.0003)	(0.0003)
	[0.0004]	[0.0004]
Additional Matching	No	Yes
Avg. # of Matched Neighbors	6.3	4.2
Plant FE	Yes	Yes
Plant-Level Clusters	193	158
Utility-Level Clusters	89	72
Divested Plants	87	81
Control Plants	106	77
R^2	0.830	0.765
Observations	16,523	13,653

Notes: Dependent variable is the logarithm of the coal price. We estimate the specification $y_{jt} = \delta_0 \cdot t + \delta_1 \cdot t \cdot \mathbb{1}(divest)_j + \gamma_j + \epsilon_{jt}$, where $\mathbb{1}(divest)_j$ is a binary indicator for whether the plant was ever divested. The data is restricted to the pre-treatment period: January 1990 to December 1997. For control plants, the pre-treatment period is defined based on its matched divested plant. Matching is based on distance and coal type based in 1997. Maximum nearest neighbors is 10. Standard errors are clustered at the plant level. Asterisks denote statistical significance at the plant level clustering * p<0.10,** p<0.05, *** p<0.01. Daggers denote statistical significance at the utility level clustering † p<0.10,†† p<0.05, ††† p<0.01.

A.4 Regression Results Using A Different Matching Criterion

This appendix shows that the main results of the paper in 4.4 are not sensitive to the choice of matching on a plant's pre-divestiture incentive regulation status. Table A.3 repeats Table 2 in the main text, but with "additional" matching that includes pre-divestiture regulatory status of each plant to distance and coal type in 1997. The estimates are very similar to the results shown in the main text.

Table A.3: Heterogeneous Effects of Divestiture on Log(Price) With Additional Matching

	(1)	(2)	(3)	(4)
$\mathbb{1}$(Divest)	$0.392^{***}_{\dagger\dagger\dagger}$	$-0.113^{***}_{\dagger\dagger}$	$0.576^{***}_{\dagger\dagger\dagger}$	-0.003
	(0.121)	(0.042)	(0.148)	(0.046)
	[0.122]	[0.056]	[0.147]	[0.061]
$\mathbb{1}$(Divest)·(Incentive years)	$0.021^{***}_{\dagger\dagger\dagger}$	$0.026^{***}_{\dagger\dagger\dagger}$		
	(0.005)	(0.005)		
	[0.007]	[0.007]		
$\mathbb{1}$(Divest)·log(Capacity)	$-0.071^{***}_{\dagger\dagger\dagger}$		$-0.095^{***}_{\dagger\dagger\dagger}$	
	(0.016)		(0.022)	
	[0.015]		[0.021]	
$\mathbb{1}$(Divest)·Disadv.·$\mathbb{1}$(Disadv.>0)	$-0.369^{***}_{\dagger\dagger\dagger}$			$-0.531^{***}_{\dagger\dagger\dagger}$
	(0.098)			(0.107)
	[0.101]			[0.118]
$\mathbb{1}$(Divest)·Disadv.·$\mathbb{1}$(Disadv.<0)	-0.126			0.124
	(0.235)			(0.253)
	[0.241]			[0.261]
Avg. # of Matched Neighbors	4.2	4.2	4.2	4.2
Year-Month FE	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes
Plant-Level Clusters	155	155	155	155
Utility-Level Clusters	76	76	76	76
Divested Plants	74	74	74	74
Control Plants	81	81	81	81
R^2	0.731	0.719	0.718	0.721
Observations	28,288	28,288	28,288	28,288

Notes: Dependent variable is the logarithm of the coal price. Capacity is at the plant level. Matching is based on distance, coal type in 1997 and pre-regulatory divestiture status of each plant. Maximum nearest neighbors is 10. Average number of matched neighbors is 6.3. Standard errors are clustered at the plant level in parentheses and at the utility level in square brackets. A plant's parent utility is determined based on plant ownership as of 1997. Asterisks denote statistical significance at the plant level clustering * p<0.10,** p<0.05, *** p<0.01. Daggers denote statistical significance at the utility level clustering † p<0.10,†† p<0.05, ††† p<0.01.

A.5 Alternative Specification for Heterogeneous Treatment Effect

In Table A.4, we show the robustness of the heterogeneous treatment effects presented in Section 4.4 with an alternative regression specification. Instead of estimating Equation (8), we estimate the following regression specification:

$$
\begin{aligned}
\log(price)_{jt} = & \alpha_0 \mathbb{1}(divest)_{jt} + \alpha_1 \mathbb{1}(divest)_{jt} \cdot \mathbb{1}(ever\ incentive)_j + \alpha_2 \mathbb{1}(divest)_{jt} \cdot \mathbb{1}(capacity > median)_j \\
& + \alpha_3 \mathbb{1}(divest)_{jt} \cdot \mathbb{1}(disadv. > 0)_j + \gamma_j + \delta_t + \epsilon_{jt}
\end{aligned}
$$

$\mathbb{1}(ever\ incentive)_j$ is an indicator variable for whether the plant j was ever under an incentive regulation program between 1990 and 1997. $\mathbb{1}(capacity > median)_j$ is an indicator variable for whether the generation capacity of the plant j is above the median capacity as of 1997. $\mathbb{1}(disadv. > 0)_j$ is an indicator variable for whether the plant j's contract is disadvantaged. Note that in a previous regression specification (Equation (8)), we interacted this term with a linear disadvantage term (i.e., $(disadv.)_j \cdot \mathbb{1}(disadv. > 0)_j$. By classifying the divested plants into two simple groups for each heterogeneous effect, we test whether a few influential observations drive the results obtained from estimating the preferred specification in Section 4.4. Though the estimates of the alternative specification are not directly comparable to those of the preferred specification, the signs of the estimates are consistent across the two specifications. We therefore conclude that the results in the main text are robust.

Table A.4: Alternative Specification For Heterogeneous Effects of Divestiture on Log(Price)

	(1)	(2)	(3)	(4)
$\mathbb{1}$(Divest)	-0.052	$-0.139^{***}_{\dagger\dagger}$	0.003	-0.022
	(0.050)	(0.048)	(0.043)	(0.040)
	[0.055]	[0.057]	[0.046]	[0.043]
$\mathbb{1}$(Divest)·(Incentive ever)	$0.112^{***}_{\dagger\dagger}$	$0.137^{***}_{\dagger\dagger\dagger}$		
	(0.041)	(0.043)		
	[0.051]	[0.052]		
$\mathbb{1}$(Divest)·1(Capacity>Median)	$-0.088^{**}_{\dagger\dagger}$		$-0.123^{***}_{\dagger\dagger\dagger}$	
	(0.042)		(0.042)	
	[0.040]		[0.037]	
$\mathbb{1}$(Divest)·$\mathbb{1}$(Disadv.>0)	-0.063			$-0.098^{**}_{\dagger}$
	(0.042)			(0.045)
	[0.048]			[0.050]
Avg. # of Matched Neighbors	6.3	6.3	6.3	6.3
Year-Month FE	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes
Plant-Level Clusters	184	184	184	184
Utility-Level Clusters	93	93	93	93
Divested Plants	80	80	80	80
Control Plants	104	104	104	104
R^2	0.785	0.781	0.781	0.779
Observations	34,145	34,145	34,145	34,145

Notes: Dependent variable is the logarithm of the coal price. Capacity is at the plant level. Matching is based on distance and coal type in 1997. Maximum nearest neighbors is 10. Average number of matched neighbors is 6.3. Standard errors are clustered at the plant level in parentheses and at the utility level in square brackets. A plant's parent utility is determined based on plant ownership as of 1997. Asterisks denote statistical significance at the plant level clustering * p<0.10,** p<0.05, *** p<0.01. Daggers denote statistical significance at the utility level clustering † p<0.10,†† p<0.05, ††† p<0.01.