Agency Frictions and Procurement: New Evidence from U.S. Electricity Restructuring

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September 2, 2021

Abstract

This paper presents new quantitative evidence of the sources of efficiency benefits from deregulation. 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 restructuring reduced fuel procurement costs for firms that (i) were not subject to earlier incentive-regulation programs, (ii) had relatively strong bargaining power as coal purchasers after restructuring, and (iii) were locked in with disadvantaged coal contracts prior to restructuring.

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

JEL codes: L5 (regulation), L94 (electric utilities), O13 (energy), Q28 (government policy)

*This is a revised version of Chapter 2 of Jin Soo Han's thesis at the Wharton School of the University of Pennsylvania. Abito thanks the Wharton Dean's Research Fund for support. Van Benthem thanks the Wharton Dean's Research Fund and the Wharton Data Science and Business Analytics Fund.

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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. Historically, the industry consisted of vertically integrated monopolists that sold power to their customers at regulated prices based on reported cost and an approved rate of return. Because of concerns that the traditional form of regulation may not be providing adequate incentives to reduce cost, state legislatures in the 1990s started acting on the idea that they could leverage market forces to deliver efficiency gains in the power sector and ultimately reduce prices for consumers. A sizable number of states passed laws to force utilities to divest their power generation assets which would then compete in wholesale markets. Transmission became the responsibility of independent system operators. Some states also introduced retail competition. The economics literature has studied various impacts of these reforms, such as competitiveness and market power in wholesale electricity markets, plant-level efficiency, generation investment, and retail competition (see Bushnell, Mansur, and Novan (2017) for a review).[1] Of key interest—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 comprised the largest portion of plants' operating expenses, the impact of restructuring on these costs was uncertain. On the one hand, restructuring was expected to deliver reductions in fuel expenditures. Upon divestiture, power plants would compete in a competitive wholesale market and become the full residual claimant on cost savings, hence providing incentives to reduce cost. On the other hand, some plants already faced some cost-minimization incentives even under the traditional regulation. Roughly 50% of plants that would eventually be divested were already subject to some form of 'incentive regulation'. 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.[2]

As utilities faced different incentives from regulations imposed 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?" To gauge the extent to which the U.S. restructuring experience can extrapolate to other settings—an open question in the literature—it is crucial to understand the underlying mechanisms through which the reform could lead to cost savings, identify potential heterogeneous effects of the reform at the firm level, and confirm if they

[1] 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).

[2] 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.

are consistent with economic theory. This paper adds new insights into how and when a power plant's divestiture can induce fuel-cost savings.[3] Our focus is on coal procurement. We present new evidence that the treatment effect of divestiture on coal procurement costs varies greatly—and consistently with economic theory—among different power plants. We identify three important channels that influence the impact of divestiture: the cost-saving incentives that a plant faced prior to divestiture, the degree of the buyer's bargaining leverage measured through the size of the buyer, and 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 an electric 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 and the quantity of coal the utility purchases (buyer size). Then, we derive that the expected change in the price paid for coal due to divestiture is decreasing in the degree of pre-divestiture cost-saving incentives 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 cost reduction to be increasing in buyer size—a condition that we argue is likely to be true in practice—and verify our predictions using empirical analysis.

In testing the model's theoretical predictions with data, we leverage the quasi-experimental variation resulting from a large number of states suspending their restructuring legislation around 2000 after the electricity crisis in California. All states considered the divestiture option as a part of their electricity sector restructuring by 1998, but only a subset of states had completed their restructuring when the California crisis hit. States that had not yet completed the process decided to revert to the prior regulatory system. Therefore, 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 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 divestiture by pre-divestiture cost-savings 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

[3] The average impact of power plant divestiture on 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. Han, Houde, van Benthem, and Abito (2020) document that most of this result was driven by a group of outlier plants owned and operated by a single electric utility and that the estimated effect becomes 6% and statistically insignificant once the outliers are accounted for. This finding does not fully answer Bushnell, Mansur, and Novan (2017). For the average plant, divestiture did not significantly improve the incentives for fuel-cost reductions that the traditional regulation had established prior to restructuring, but the above-mentioned papers do not distinguish plants based on the type of pre-existing regulation that they faced.

in the earlier periods had been 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).[4] 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%; plants with favorable contracts did not experience any cost savings. This suggests that divestiture increased the benefits of contract renegotiation.

We conclude that the restructuring of the U.S. power 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 firms with strong incentives to renegotiate existing contracts or with relatively more bargaining leverage over coal mines.[5] Other divested plants do not achieve any cost savings at all. In the long run, as contracts expire, one might reasonably expect that the effect of divestiture will grow for at least a subset of plants—divested plants that were initially unsuccessful at renegotiating unfavorable contracts will have strong incentives to negotiate hard once their contracts expire and their deregulated status will entitle them to the full cost savings.

Our empirical findings are in line with the theory of economic regulation and 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, and which types of plants should be expected to achieve cost savings. Our paper focuses on divestiture as a potential mechanism that encouraged plants to seek new coal contracts or to renegotiate existing ones, either through voluntary means that mutually benefit the buyer and seller, or through involuntary means where the buyer cites *force majeure* and gross inequity as reasons for renegotiation (Joskow, 1990).[6] Our finding that firms respond differentially to the incentives created by market reforms—and therefore how market efficiency could be enhanced by restructuring—allows regulators and policymakers to better predict which firms would respond to a proposed change in rules. This is especially important for the external validity of the deregulation studies about the United States and the extrapolation of U.S. experiences to other states and countries that consider restructuring their power markets.

[4]Our model provides a new insight to the theory of 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.

[5]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).

[6]For example, a divested plant may argue that divestiture eliminated the ability to pass through costs through regulated electricity prices and an unfavorable pre-divestiture contract puts the plant at a gross cost disadvantage relative to other plants it competes against.

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 these hypotheses empirically and report the estimation results. We 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 cover a utility's "prudently" incurred operating costs.

Although cost-of-service regulation, in theory, provided regulators the ability to disallow imprudently incurred expenses, in practice, information asymmetries and costly verification (i.e., evidence of impropriety have to be presented in a quasi-judicial process called a rate case) essentially allowed complete pass through of costs via 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).

While incentive regulation broadly includes many different programs, the types of incentive regulation programs that encourage procurement cost savings are variants of regulation that contain fuel-adjustment clauses (FACs), a price cap, a rate freeze, or an earnings-sharing agreement. 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 the traditional FACs.[7] As state regulators eliminated fuel-adjustment clauses or replaced them with a modified fuel-adjustment clause that does not fully pass through cost overruns, utilities began 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 was also aimed at reducing the

[7] 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.

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. 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.[8] Finally, an earnings-sharing program allows utilities to retain some of the gains from the reductions in operating costs such as fuel procurement.[9]

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 metrics 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 broader national effort was initiated by the federal government in the mid-1990s to make the electricity sector more competitive.[10] The effort was mostly focused on introducing competition in the generation segment of the electricity sector by splitting generation from transmission and distribution.[11] Although actual implementation was up to states' discretion, most states were interested in the idea of market restructuring, and all had 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 substantial 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.[12] 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 gen-

[8] MT, NY, OR, and WA implemented price-cap regulation for certain utilities. LA, MO, and NY implemented a rate freeze for certain utilities.

[9] Under a rate regulation, regulators typically set an allowed return on equity. An earnings-sharing program allows utilities to keep a certain proportion of the excess earnings made above this allowed return on equity and share (or return) the rest with the customers. If a utility reduces fuel-procurement costs under an earning-sharing program, then it will reap some portion of the benefits of cost savings. CT, IA, IN, LA, MA, MO, MT, NY, OH, and OR implemented earnings sharing for certain utilities.

[10] 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, 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 regional transmission organizations and independent system operators (Order No. 2000 in 1999) that would manage wholesale electricity markets.

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

[12] 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 division in such cases.

eral, several studies have exploited this natural experiment in which 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.[13]

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 (Knittel, 2002). 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 deregulation 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 terms of these contracts specify the price, quantity, and quality of the delivered coal.

Given the long-term nature of these contracts, small adjustments in contracted quantities and quality are allowed and explicitly specified in the contract. The contract also allows for changes in price based on an adjustment formula that tracks various input price indices across time. Joskow (1988) notes that the adjustments embedded in the contract allowed the contracted price to adequately respond to input price increases experienced in the 1970s and early 1980s such that contract breach or renegotiation were minimal. However, these adjustments were less effective against negative demand shocks that drove down the market price for coal in the late 1980s and after, leading to increased renegotiation, buy-out and litigation activity (Joskow, 1990).

There are several ways through which restructuring and divestiture can affect long-term contracts and coal procurement costs. Divested plants face stronger incentives to reduce cost since they cannot recoup all costs through regulated electricity prices. This provides incentives to exert effort to seek new contracts with better terms, or to renegotiate or even break existing ones. Re-

[13] Researchers have cautioned against a potential selection bias of this estimation strategy because states with high electricity prices were more likely to restructure (White, Joskow, and Hausman, 1996) but in practice the high electricity prices in certain states 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 invest.

structuring can also be used to support involuntary renegotiation or in defense of contract breach by citing *force majeure* or gross inequity (Joskow, 1990) and claiming that renegotiation or breach is the only way for the divested plant to compete and survive in the market.

We focus on three mechanisms that can affect the impact of divestiture on coal procurement costs. First, utilities facing incentive-regulation programs prior to divestiture may experience a smaller reduction in costs from divestiture. To the extent that these incentive-regulation programs are already effective at inducing effort to seek favorable contracts, the chance of finding a much better contract after divestiture is smaller, all else equal.

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'. 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 renegotiation.

With respect to buyer size, the countervailing bargaining 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 on 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 power-generation capacity is relevant for its role as a coal buyer when the suppliers typically invest in large relationship-specific assets in anticipation of the deal (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. In our model, we allow size to affect both pre and post-divestiture contracts.

Finally, we examine how a plant's pre-divestiture contractual terms influence the effect of divestiture on coal procurement costs. Plants with pre-divestiture contracts that were less favorable compared to those received by similar plants will have a greater desire to renegotiate their contracts since it puts the plant at a greater cost disadvantage relative to other rival plants after divestiture. Moreover, the expected benefit of renegotiation is higher for plants with less favorable pre-divestiture contracts, all else equal.

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 the negotiated coal price p_i be

$$U_i = s_i(v - \gamma_i p_i)$$

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 is passed on to consumers through higher regulated electricity prices.[14] The parameter γ_i would then be the fraction of costs retained by the utility, for instance, under incentive regulation. In the case of full cost pass-through, the parameter γ_i is equal to zero.

Before arriving at the negotiated price p_i, the utility has to decide on the level of negotiation effort to exert, e_i. 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 $\psi()$ 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

$$E(U_i) = s_i \left(v - \gamma_i E[p_i|e_i] \right) - \psi(e_i).$$

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 $\psi'(e_i)$. First, observe 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.[15]

3.1 The Effect of Divestiture

Upon divestiture, a utility fully absorbs the costs of fuel such that its cost-retain rate γ_i becomes 1. As discussed in Section 2, we assume that divestiture potentially opens up the ability to renegotiate existing contracts. 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

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

[14] The parameter γ captures in a reduced-form way the equilibrium interaction between the regulator and the utility in the price-setting process.

[15] We assumed for convenience that the cost of effort is independent of size. As long as the effect of size on the marginal benefit of effort is larger than its effect on the marginal cost of effort, then optimal effort is increasing in size.

where e_i^* is the optimal post-divestiture level of effort. The left-hand side of the inequality is the utility's payoff if it continues with its pre-divestiture contract while the right-hand side is the utility's expected payoff if it chooses to renegotiate.

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 which we define as the difference between the utility's ex-post pre-divestiture price and its expected price conditional on pre-divestiture effort e_{0i}^*:

$$D_i = p_{0i} - E[p_{0i}|e_{0i}^*].$$

We denote the probability of renegotiation after divestiture by

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

where

$$\hat{\phi}_i = s_i \left(D_i + E[p_{0i}|e_{0i}^*] - E[p_i|e_i^*] \right) - \psi(e_i^*).$$

Consider first γ_{0i}. The partial derivative of μ with respect to γ_{0i} is

$$\frac{\partial \mu}{\partial \gamma_{0i}} = -\frac{dG}{d\phi_i} \left(s_i \left| \frac{dE[p_{0i}|e_{0i}^*]}{de} \right| \frac{\partial e_{0i}^*}{\partial \gamma_{0i}} \right).$$

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_{0i}), the utility would have exerted considerable 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

$$\frac{\partial \mu}{\partial s} = \frac{dG}{d\phi} \left\{ (p_{0i} - E[p_i|e_i^*]) - s_i \left| \frac{dE[p_{0i}|e_{0i}^*]}{de} \right| \frac{\partial e_{0i}^*}{\partial s} \right\}. \quad (1)$$

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, at least when $\gamma_{0i} > 0$. There are two channels through 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. For the case of full cost pass-through however, pre-divestiture effort is zero for all s_i hence the second channel disappears and the probability of renegotiation is unambiguously increasing in size.

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

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

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

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

$$\Delta(\gamma_{0i}, s_i, D_i) = \mu_i \left(D_i + E[p_{0i}|e_{0i}^*] - E[p_i|e_i^*] \right)$$

where $\mu_i = \mu(\gamma_{0i}, s_i, D_i)$ is the probability of renegotiation. The following proposition presents the comparative statics on $\Delta_i = \Delta(\gamma_{0i}, s_i, D_i)$ with respect to the variables of interest. These comparative statics provide predictions that we test empirically in the next section.

Proposition 1. *The expected price change due to divestiture is decreasing in the pre-divestiture incentive γ_{0i} and increasing in the degree of contract disadvantage D_i. As long as the elasticity of the probability of renegotiation with respect to size satisfies the inequality*

$$\frac{\partial \mu}{\partial s} \frac{s_i}{\mu_i} \geq 1 - \frac{\left| \frac{dE[p_i|e_i^*]}{de} \right| \frac{\partial e_i^*}{\partial s}}{\left| \frac{dE[p_{0i}|e_{0i}^*]}{de} \right| \frac{\partial e_{0i}^*}{\partial s}}, \tag{2}$$

the expected price change is increasing in size. In the case of full cost pass-through, this sufficient condition is satisfied and the expected price change is increasing in size.

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

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

and

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

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:

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

Observe that from Equation (1),

$$\frac{\partial \mu}{\partial s} > 0 \iff p_{0i} - E[p|e_i^*] > s_i \left|\frac{dE[p_{0i}|e_{0i}^*]}{de}\right| \frac{\partial e_{0i}^*}{\partial s}$$

hence

$$\frac{\partial \mu}{\partial s}\left(p_{0i} - E[p_i|e_i^*]\right) > \frac{\partial \mu}{\partial s} s_i \left|\frac{dE[p_{0i}|e_{0i}^*]}{de}\right| \frac{\partial e_{0i}^*}{\partial s}.$$

This implies

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

Thus, it suffices that

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

in order for $\frac{\partial \Delta}{\partial s} > 0$. We can rewrite this in terms of the elasticity of the probability of renegotiation with respect to size as shown in the proposition:

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

Finally, note that when there is full cost pass-through, pre-divestiture effort e_{0i}^* is always zero for any s hence the sufficient condition is satisfied. □

Our model predicts that the expected price change due to divestiture is decreasing in pre-divestiture cost-savings incentives and increasing in pre-divestiture contract disadvantage. The effect of size on the expected price change is ambiguous except for the case of full cost pass-through. Nevertheless, the proposition provides a sufficient condition based on the probability of renegotiation such that the expected price change due to divestiture is increasing in size. Although we cannot check whether the sufficient condition holds since we do not have data on renegotiations, we can check if the pre-divestiture price is weakly correlated with size. A weak correlation implies that the term $\left|\frac{dE[p_{0i}|e_{0i}^*]}{de}\right| \frac{\partial e_{0i}^*}{\partial s}$ is close to zero and therefore the inequality for the sufficient condition is satisfied. In this case, our model predicts a positive relationship between the expected price change due to divestiture and size.

4 Empirical analysis

4.1 Empirical Model & Data

Our primary goal is to understand the mechanisms through which deregulation delivers fuel-cost reductions. In the previous section, we have derived a core set of predictions about how the impact of power plant divestiture varies by a plant's pre-divestiture cost-savings incentives, size, and contract disadvantage. We now explain how we test these theoretical predictions empirically. The following is the estimating equation that we bring to the data:

$$\begin{aligned}\log(price)_{jt} =& \alpha_0 \mathbb{1}(divest)_{jt} + \alpha_1 \mathbb{1}(divest)_{jt} \times (incentive\ years)_j + \alpha_2 \mathbb{1}(divest)_{jt} \times log(capacity)_j \\ &+ \alpha_3 \mathbb{1}(divest)_{jt} \times (disadv.)_j \times \mathbb{1}(disadv. > 0)_j \\ &+ \alpha_4 \mathbb{1}(divest)_{jt} \times (disadv.)_j \times \mathbb{1}(disadv. \leq 0)_j + \gamma_j + \delta_t + \epsilon_{jt}\end{aligned} \tag{3}$$

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 difference-in-differences (DiD) indicator for whether plant j is divested and whether time t is after the year-month of the divestiture. $(incentive\ years)_j$, $log(capacity)_j$ and $(disadv.)_j$ are the moderating variables that empirically test whether the effect of deregulation on the delivered coal prices varies by a plant's incentive regulation, size, and contract disadvantage. The variable $(incentive\ years)_j$ measures the number of years a plant is subject to incentive regulation between 1990 and 1997 where the type of incentive regulation programs we consider are 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.

Given the estimating Equation (3), we can now directly test the predictions of the theory model by looking at the sign of the estimated coefficients. First, since our theory model predicts that the expected cost reduction from divestiture is smaller for plants with more pre-divestiture cost-savings incentives, we expect the coefficient α_1 on $\mathbb{1}(divest)_{jt} \times (incentive\ years)_j$ to be positive. $\alpha_1 > 0$ implies that plants subject to a longer period of pre-divestiture incentive regulation achieve lower cost reductions compared to plants subject to a shorter period of such regulation.

Our theory model then predicts that the expected price change from divestiture is larger for larger plants if the probability of renegotiation is sufficiently elastic with respect to size (i.e., inequality (2) in Proposition 1). Unfortunately, while this condition is trivially satisfied in theory when a plant's pre-divestiture effort (i.e., e^*_{0i}) is always equal to zero in the case of full cost pass-through, we cannot directly test the condition because we do not observe the level of effort plants exert for contract negotiations. We instead investigate the correlation between pre-divestiture price and size. A weak correlation between pre-divestiture price and size implies that the elasticity condition is satisfied since the right-hand side of inequality (2) diverges to negative infinity. With this purpose in mind, we estimate the following regression specification:

$$log(price)_{jt} = \beta_1 log(capacity)_{jt} + \gamma_j + \delta_t + \epsilon_{jt}$$

We restrict our data to the pre-divestiture sample excluding the years after 1997. The estimated coefficient on $log(capacity)_{jt}$ is 0.051 but statistically insignificant with a t-statistic of 0.97. Even when we allow for cross-plant variation by removing the plant fixed effects, the estimated coefficient on $log(capacity)_{jt}$ becomes 0.022 and is still statistically insignificant with a t-statistic of 1.05. Therefore, we expect cost reductions from divestiture to be larger for larger plants and the sign of size coefficient α_2 in Equation (3) to be negative (i.e., $\alpha_2 < 0$).

Lastly, the proposition of the theory model states that the expected cost reduction upon divestiture is larger for plants with more pre-divestiture contract disadvantage. Because utilities can have favorable contracts (i.e., $(disadv.)_j \leq 0$), we allow for an asymmetric response to divestiture by interacting it with the indicators $\mathbb{1}(disadv. > 0)_j$ and $\mathbb{1}(disadv. \leq 0)_j$. Note that if $\alpha_3 = \alpha_4 < 0$ in Equation (3), 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. If divestiture serves as an impetus to renegotiate and improve contracts for plants stuck with unfavorable contracts, we expect α_3 to be negative but we also expect divestiture not to have the same effect on plants with favorable contracts and hence $\alpha_4 = 0$.

In order to test our empirical model in Equation (3), we collect data from various sources. First, we merge several survey forms collected by the Energy Information Administration (EIA) to gather information on a plant's fuel expenditures, characteristics, and divestiture status (Energy Information Administration, 1990-2009). We supplement the divestiture data with the Environmental Protection Agency's (EPA) data on changes to a plant's ownership (Environmental Protection Agency, 1996-2012). To test our model's main predictions, we use EIA data to construct a plant's size and a measure of contract disadvantage. We rely on the Regulatory Research Associates' (RRA) *Regulatory Focus* reports for information on a utility's pre-divestiture regulatory details (Regulatory Research Associates, 1990-1998). 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.[16] We direct the reader to Appendix A.1 for the specific survey forms of EIA, EPA, and RRA collected for the data.

[16]We note a caveat to the data. A part of the expenditure data is missing for the divested plants. The EIA did not require divested plants to report their fuel expenditures once they became divested. The agency resumed 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 measures the differences in the outcome variable before and *two-years-after* the treatment as opposed to before and (immediately) after.

Before we proceed to motivate our research design, we provide a rationale for our empirical choices for the moderating variables. First, for a plant's incentive regulation, we classify the plant 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. Other alternative regulatory methods such as a heat-rate program or an Equivalent Availability Factor program focus on increasing a plant's production efficiencies.[17] We choose the types of incentive regulation programs that provide direct incentives for fuel-cost savings as discussed in Knittel (2002).

For buyer size, we use a plant's generation capacity as a proxy. In theory, the amount of coal a plant purchases does not necessarily equal the quantity of coal the plant 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 occasional maintenance), suggesting that a plant's generation capacity is a *de facto* measure for its coal consumption. We thus define buyer size to be a plant's generation capacity as of 1997.

The pre-divestiture contract disadvantage in our theory 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 contracted coal prices negotiated by their neighbors conditional on *every* possible level of effort, i.e. both equilibrium and off-equilibrium 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 six 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 in 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.3, we show that our results are robust to extending our matching criteria 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.

4.2 Research Design

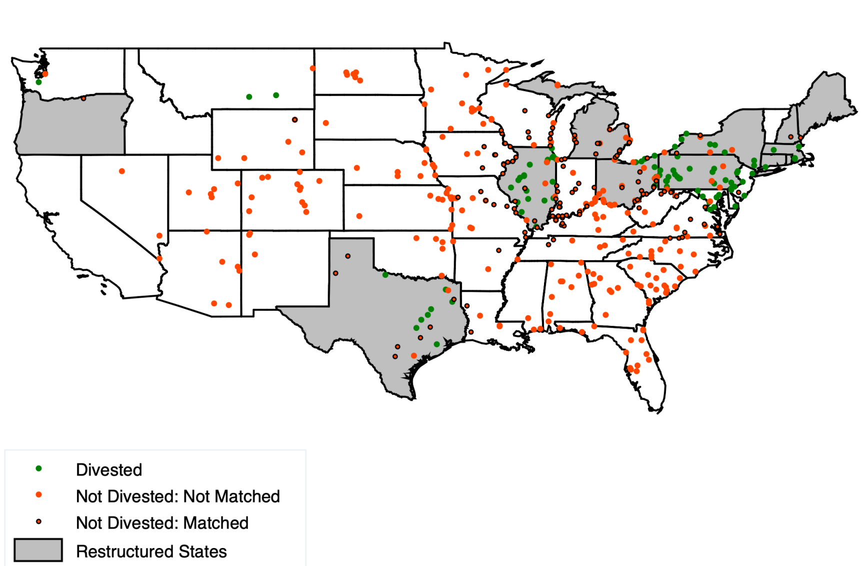
In estimating our empirical model in Equation (3), we employ a *matched* difference-in-differences estimator similar to the synthetic control approach in Abadie, Diamond, and Hainmueller (2012). The treatment group consists of divested plants in restructured states, while the control group

[17]For example, the heat-rate programs typically set prices based on a utility's average heat rate (i.e., the amount of energy used to generate a megawatt hour of electricity). The firm retains the benefits if it operates at a more efficient heat rate than the pre-specified average rate.

consists of regulated plants that remain under the status quo cost-of-service regulation. We match each divested plant to (at most) the 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.

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., the 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.

Before further examining the empirical validity of our research design, we note that we follow Han et al. (2020) and drop seven outlier power plants from the sample in the remainder of this paper. These outlier plants belong to a single utility and are influenced by an event unrelated to treatment in the pre-treatment period and contribute to more than half of the average treatment effect of divestiture on fuel costs. In Appendix A.2, we discuss the presence of outliers and show that our heterogeneous-treatment results are robust to including the outliers. We refer the reader to the aforementioned paper for the details of outlier detection.

We then examine the validity of our research design. We conduct two standard tests of a DiD estimator on a matched sample: a balance-of-covariates test and a test for equal pre-trends. In Table 1, we examine whether divested and non-divested plants are similar along a number of observable characteristics. The two groups are indeed similar conditional on matching, which allows the econometrician to compare plants with similar fuel-procurement conditions based on coal type and geography. Heat, sulfur and ash contents of coal show that divested and non-divested plants purchase similar-quality coal on average. Divested and non-divested plants also source coal from similarly-distanced mines under similarly-priced contracts.

We now proceed to a pre-trend test in which we plot the outcome variable of interest over time for the treatment and the control group and gauge whether the two groups exhibit any differential trends. Since we delve into heterogeneous treatment effects, we visualize pre-trends not only by treatment and control but also by the moderating variables. In Figure 2, we plot the difference in logged delivered coal prices between divested and non-divested plants by (i) whether a plant is ever subject to pre-divestiture incentive regulation, (ii) whether a plant's capacity is above the median, and (iii) whether a plant has a disadvantaged contract. We first plot the simple difference between treatment and control plants without any moderating variables in Panel (a). The flat line reveals no visual evidence of a differential time trend in logged prices between divested and non-divested plants in the pre-period. In Panels (b), (c), and (d), we further decompose the average prices by the moderating variables. Though relatively more noisy, Panels (b), (c) and (d) all show that different subgroups of divested and non-divested plants based on a respective moderating variable share similar pre-trends. At least graphically, there does not seem to be an endogenous factor that affects a particular subgroup of plants differentially in the absence of treatment.

We also formally test whether divested and non-divested plants have differential linear trends before 1997 using the following specification:

$$log(price)_{jt} = \delta_0 Time_t + \delta_1 Time_t \times \mathbb{1}(divest)_j + \gamma_j + \epsilon_{jt}$$

where $Time_t$ indicates a year-month of the sample period t (i.e., a linear time trend), $\mathbb{1}(divest)_j$ is an indicator variable for whether the plant is ever divested, and γ_j denotes plant fixed effects. A coefficient of -0.0001 on $Time_t \times \mathbb{1}(divest)_j$ reported in Table 2, Column (1), suggests that there is a slight negative pre-trend of -0.1 percentage points per month, but the slope coefficient is not statistically significant (t-statistic: -0.37). To test for differential linear trends for each subgroup of divested and non-divested plants based on a moderating variable, we estimate the following regression specification:

$$\begin{aligned} log(price)_{jt} = {} & \delta_0 Time_t \times Moderator_{j,0} + \delta_1 Time_t \times \mathbb{1}(divest)_j \times Moderator_{j,0} \\ & + \tilde{\delta}_0 Time_t \times Moderator_{j,1} + \tilde{\delta}_1 Time_t \times \mathbb{1}(divest)_j \times Moderator_{j,1} + \gamma_j + \epsilon_{jt} \end{aligned}$$

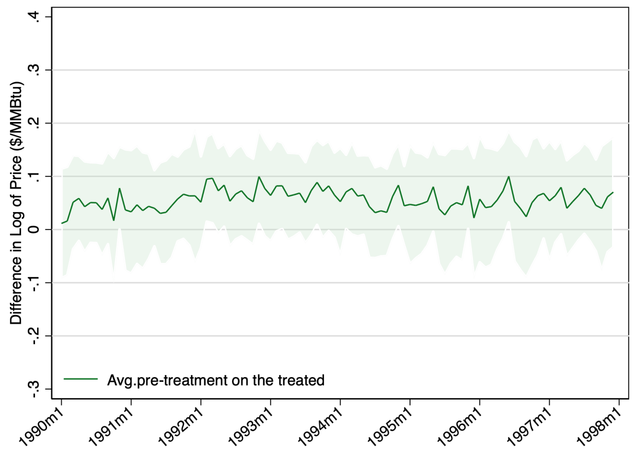
where $Moderator_{j,1}$ is one of the following indicator variables: (i) whether a plant is ever subject to pre-divestiture incentive regulation; (ii) whether a plant's capacity is greater than a median plant;

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

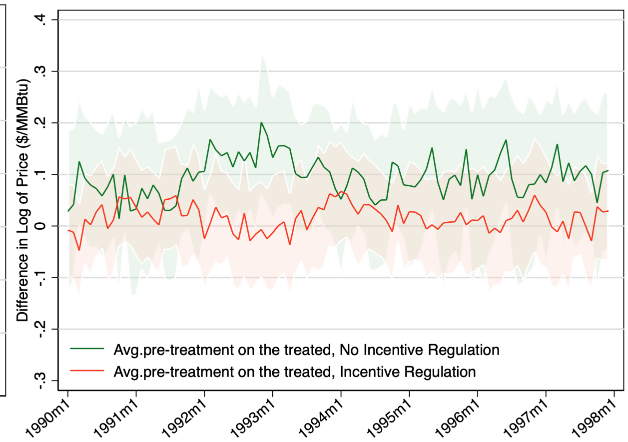
	(1)	(2)	(3)
	Divested	Not divested	Difference
Panel A: Time-invariant statistics as of 1997			
Plant vintage	1960.38	1960.10	0.28
	[11.85]	[15.37]	(3.09)
Pre-divestiture contract disadvantage	0.02	-0.01	0.03
	[0.22]	[0.19]	(0.05)
Years of incentive regulation	2.41	1.83	0.59
	[3.34]	[3.21]	(0.65)
Panel B: Monthly average statistics between 1990 and 1997			
Log(Price ($/MMBtu))	0.35	0.30	0.05
	[0.24]	[0.24]	(0.04)
Log(Price under contracts ($/MMBtu))	0.38	0.33	0.05
	[0.25]	[0.24]	(0.04)
Millions MMBtu Delivered	3.71	3.15	0.56
	[3.64]	[3.30]	(0.61)
Percent bituminous	0.85	0.83	0.01
	[0.35]	[0.36]	(0.07)
Percent sub-bituminous	0.07	0.08	-0.01
	[0.25]	[0.26]	(0.04)
Heat content (MMBtu/ton)	23.47	23.14	0.33
	[3.91]	[3.94]	(0.76)
Sulfur content (lbs/MMBtu)	1.51	1.65	-0.14
	[0.81]	[0.96]	(0.15)
Ash content (lbs/MMBtu)	10.11	10.25	-0.14
	[6.02]	[7.13]	(1.17)
Distance to mine (mi.)	217.14	206.37	10.78
	[232.65]	[242.65]	(38.67)
Annual capacity (MW)	975.96	886.30	89.67
	[763.71]	[719.96]	(141.56)
Annual capacity factor	0.52	0.49	0.04
	[0.18]	[0.21]	(0.04)
Plants	80	105	185

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.

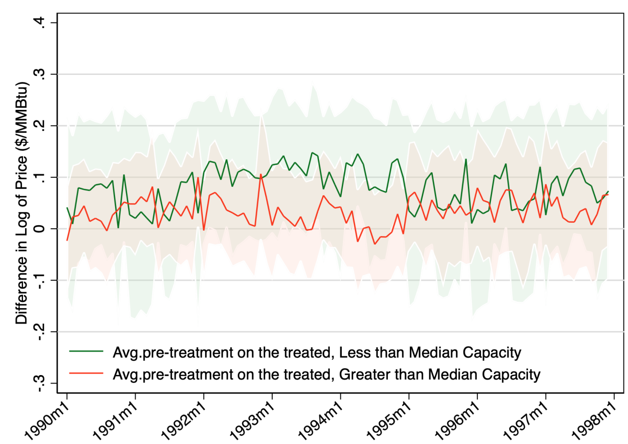
Figure 2: Pre-Trends in Delivered Coal Prices



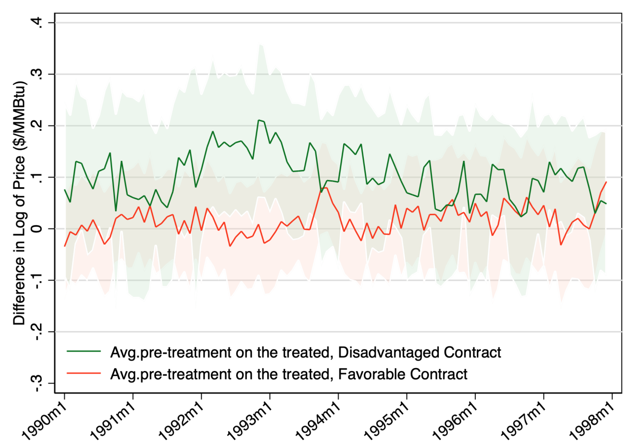
(a) Price Difference



(b) Price Difference by Incentive Regulation



(c) Price Difference by Median Capacity



(d) Price Difference by Contract Disadvantage

Notes: Panel (a) shows the difference in the weighted average of log delivered coal costs in $/MMBtu between divested and non-divested plants. Panel (b) shows the difference in the weighted average of log costs by whether a plant is ever subject to incentive regulation in the pre-divestiture period. Panel (c) shows the difference in the weighted average of log costs by whether a plant's capacity in 1997 is greater than the median. Panel (d) shows the difference in the weighted average of log costs by whether a plant has disadvantaged or favorable contracts as of 1997. In all of the 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 ($\log(cost)_{jt} = \gamma_t + \gamma_t 1(Divest)_j + \epsilon_{jt}$) clustered at the plant level.

or (iii) whether a plant has a disadvantaged contract. $Moderator_{j,0}$ is equal to 1 if $Moderator_{j,1}$ is 0 and vice versa in order to indicate the subgroup of plants that does not meet the criterion of a moderating variable (i.e., a complement set of $Moderator_{j,1}$). For incentive regulation, statistically insignificant coefficients on $Time_t \times \mathbb{1}(divest)_j \times Moderator_{j,0}$ and $Time_t \times \mathbb{1}(divest)_j \times Moderator_{j,1}$ in Column (2) show that the subgroup of divested plants *not* subject to pre-divestiture incentive regulation and the subgroup of divested plants subject to incentive regulation do not exhibit differential pre-trends compared to their non-divested counterparts. We find no evidence of differential linear trends for different subgroups using other moderator variables in Columns (3) and (4) either. Thus, we conclude that neither the balance-of-covariates test nor the pre-trend tests raise an apparent concern for adopting a DiD research design.

Table 2: Hypothesis Tests on Linear Pre-Trends

	(1) Moderator: None	(2) Moderator: Incentive Regulation	(3) Moderator: Capacity	(4) Moderator: Contract Disadvantage
Time	$-0.0015^{***}_{\dagger\dagger\dagger}$			
	(0.0002)			
	[0.0002]			
Time$\times$ $\mathbb{1}$(Divest)	-0.0001			
	(0.0003)			
	[0.0003]			
Time $\times$ Moderator$_{j,0}$		$-0.0016^{***}_{\dagger\dagger\dagger}$	$-0.0013^{***}_{\dagger\dagger\dagger}$	$-0.0010^{***}_{\dagger\dagger\dagger}$
		(0.0003)	(0.0003)	(0.0003)
Time $\times$ Moderator$_{j,1}$		$-0.0012^{***}_{\dagger\dagger\dagger}$	$-0.0016^{***}_{\dagger\dagger\dagger}$	$-0.0018^{***}_{\dagger\dagger\dagger}$
		(0.0003)	(0.0004)	(0.0003)
Time $\times$ $\mathbb{1}$(Divest)		0.0000	-0.0001	0.0000
$\times$ Moderator$_{j,0}$		(0.0005)	(0.0004)	(0.0004)
Time $\times$ $\mathbb{1}$(Divest)		-0.0004	-0.0001	-0.0002
$\times$ Moderator$_{j,1}$		(0.0004)	(0.0005)	(0.0004)
Avg. # of Matched Neighbors	6.1	6.1	6.1	6.1
Plant FE	Yes	Yes	Yes	Yes
Plant-Level Clusters	172	172	172	172
Utility-Level Clusters	84	84	84	84
Divested Plants	80	80	80	80
Control Plants	92	92	92	92
R^2	0.812	0.812	0.812	0.815
Observations	14,860	14,860	14,860	14,860

Notes: Dependent variable is the logarithm of the coal price. In Column (1), we estimate the specification $y_{jt} = \delta_0 t + \delta_1 t \mathbb{1}(divest)_j + \gamma_j + \epsilon_{jt}$, where $\mathbb{1}(divest)_j$ is a binary indicator for whether the plant was ever divested. In Columns (2), (3), and (4), we estimate $log(price)_{jt} = \delta_0 Time \times Moderator_{j,0} + \delta_1 Time \times \mathbb{1}(divest)_j \times Moderator_{j,0} + \tilde{\delta}_0 Time \times Moderator_{j,1} + \tilde{\delta}_1 Time \times \mathbb{1}(divest)_j \times Moderator_{j,1} + \gamma_j + \epsilon_{jt}$ where $Moderator_{j,1}$ is one of the following indicator variables: (i) whether a plant is subject to pre-divestiture incentive regulation; (ii) whether a plant's capacity is above the median plant; or (iii) whether a plant has a disadvantaged contract. $Moderator_{j,0}$ is equal to 1 if $Moderator_{j,1}$ is 0 and vice versa indicating a complement set of $Moderator_{j,1}$. 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 in parentheses and at the utility level in square brackets. Asterisks denote statistical significance using plant-level clustering * p<0.10,** p<0.05, *** p<0.01. Daggers denote statistical significance using utility-level clustering † p<0.10,†† p<0.05, ††† p<0.01.

As for the estimation, we employ weighted least squares (WLS) at the plant-month level—not the utility-month level—and cluster standard errors at the utility level.[18] We treat the plant as our unit of analysis. However, because there is anecdotal evidence that contracts for multiple plants can be jointly negotiated by a single operating company (Han et al., 2020), we cluster standard errors at the utility-month level.[19]

Despite our matching procedure to reinstate similar procurement conditions for the treatment and the control group, one may raise a reasonable concern that local utilities subject to different incentive-regulation programs face different cost-saving incentives. We show the robustness of our results by adding the pre-divestiture regulatory status of each plant to the set of matching criteria, which we refer to as 'additional matching'. Appendix A.3 shows the robustness of the validity of pre-trend tests and the regression results with additional matching.[20]

4.3 Heterogeneous Treatment Effect Estimates

We now present the results of estimating our regression model. Column (1) of Table 3 shows the regression estimates of Equation (3). We also estimate specifications where we treat each mechanism individually in Columns (2) to (4).[21] 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. Column (2) suggests that 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; firms that faced incentive regulation for six or more years during the pre-period did not reduce their costs at all. In Table A.5 in Appendix A.4, we decompose the treatment effect by an indicator variable for whether plants were ever subject 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 than that for the divested plants never subject to incentive regulation.[22] Our findings illustrate

[18]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).

[19]We do not collapse the data to the utility-month level for three reasons. The first 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 (Energy Information Administration, 1993-2001) reveal that a contract is responsible for deliveries to 1.7 plants on average with a median of 1 plant. Third, 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.

[20]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).

[21]In Appendix A.2, we present the robustness of the heterogeneous treatment effect estimates by keeping the outlier plants in the data.

[22]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.,

Table 3: 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 using plant-level clustering * p<0.10,** p<0.05, *** p<0.01. Daggers denote statistical significance using utility-level clustering † p<0.10,†† p<0.05, ††† p<0.01.

that cost reductions from divestiture depend strongly on the prior regulatory regime and that new market pressures provided limited incentives for plants that are 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 establish that the divestiture effect is larger for larger plants in the case of full cost pass-through when plants exert no effort for contract negotiations regardless of size. Empirically, we have found evidence consistent with zero effort (i.e., no correlation between price and size prior to divestiture) and therefore we expect a post-divestiture size effect to be present.

The regression estimates support that the response to divestiture was indeed highly concentrated among larger plants. In Appendix Table A.5, 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 above the median reduced fuel costs by about 12.0%-14.0% while the divested plants with below-median capacity achieved no cost savings. These results support our hypothesis that effort and size are likely complementary in eliciting a better price offer when 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-restructuring. The last column in Table 3 suggests that their cost even increased, although the coefficients are too imprecisely estimated to draw any meaningful conclusions. In Appendix Table A.5, we decompose the treatment effect 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 their 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 (or force the renegotiation of unfavorable contracts in the courts), but plants with favorable contracts upheld the existing contractual terms. This result provides further evidence for a mechanism through which divestiture delivers a reduction in fuel expenditure.

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 in their fuel-

$((-0.114) + (-0.1140.0215))/2)$ though we find this estimate to be statistically insignificant (Han et al., 2020).

procurement cost, confirming that pre-divestiture incentive regulations were effective at eliminating the moral hazard problem associated with fuel-cost negotiation. Similarly, plants that in theory had 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—and thus higher bargaining leverage—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. Because changes in regulatory regimes such as mandatory divestiture could invoke *force majeure* or gross inequity claims for contract renegotiations, an option to source coal from different suppliers became a credible threat and therefore improved the bargaining position of power plants relative to coal mines. Divestiture can also encourage voluntary renegotiations. Both the seller and the buyer could benefit if the marginal cost of the seller is higher than the market price of coal. In this case, a plant could buy out its contract at a cost that is higher than the lost profits to the mine, but lower than the value of a new contract. A divested plant had a clear incentive to take advantage of this mutually beneficial renegotiation while a regulated plant that can fully pass through costs did not.

However, the presence of contract buy-outs brings us to a caveat in interpreting the results. Some of the estimated cost savings from divestiture are offset by transfers. When divested plants compensate for (i.e., 'buy out') not-yet-expired contracts, the price of delivered coal is negotiated down but in exchange for a financial commitment unobserved to the researcher.[23] Therefore, while some of the estimated cost reductions reflect an improved (incentive to exploit) bargaining power, a part of these gains could have been transferred back to coal mines. In that case, the estimated 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 that power plants face upon divestiture vary substantially. When firms' incentives differ significantly within the same local market, the magnitude of fuel-cost savings from deregulation remains an empirical question for each institutional and geographical setting—different segments of the power-generation industry, or power markets in different countries. While the lessons from the U.S. electricity sector restructuring may not carry over to other settings, our results allow for more meaningful extrapolation using another market's distribution of pre-existing regulation, plant size and contract (dis)advantage. The results in this paper therefore not only contribute to our understanding of the mechanisms through which market forces can lead to cost reductions, but also augment the external validity of estimates obtained for particular markets.

[23] A lump-sum payment or a future purchase commitment was not an uncommon practice in divestiture deals. For example, the divestiture of ComEd plants in Illinois involved a lump-sum settlement payment to the new buyer for the high-cost coal contracts that the old buyer signed.

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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. Prior to 2008, EIA and FERC collected the cost data for non-utilities and utilities separately. The survey forms EIA-423 and FERC-423 were unified in 2008 as EIA-923. All of the survey forms are managed by and can be obtained from the EIA. 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 a 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 changes 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 M&A data from Thomson Reuters. Divestiture dates can be inferred from the changes in plant operators. While EIA-906, *eGRID* and the M&A 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, EIA-860, and US census Bureau 2016 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.

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.

Finally, the *Coal Transportation Rate Database* provides data on individual coal contracts at the utility level. We use the data to calculate the average number of plants per operator reported in the main text.

A.2 Robustness to the Presence of Outliers

In the data section of the paper, we note that we drop seven outlier plants. In this appendix, we first explain why we classify these seven plants as outliers and then show that our heterogeneous-treatment results are robust regardless of the presence of the outliers. Following the recent applied econometrics literature that questions the statistical power of the standard pre-tests (Roth, 2018; Freyaldenhoven, Hansen, and Shapiro, 2019), Han et al. (2020) 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 is not immediately revealed by standard pre-trend 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. ComEd had renegotiated its coal contracts before—and for reasons unrelated to—restructuring; 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.

While there are several options to account for the presence of such problematic outliers, our preferred specification is to drop them from the sample. Doing so results in a statistically-insignificant average treatment effect of divestiture on fuel costs of about 6%.[24] This estimate is not sensitive to a particular choice of how we account for the outliers—robust regression techniques such as median regression yield almost the same coefficient.[25] In this appendix, we confirm that the presence of outliers does not strongly affect the heterogeneous treatment effects estimated reported in the main text.

First, we revisit the standard pre-trend tests keeping the outliers in the sample. In Appendix Table A.1, we report the summary statistics of the observable characteristics for divested and non-divested plants. Though divested and non-divested plants are similar in observables across the board, the covariates related to the delivered coal prices differ substantially between the two groups. For example, plants in the treated sample paid 10-12% higher coal prices in the pre-period. This is the first hint of outlier plants in the sample. But at this stage, such a price difference between divested and non-divested plants is not necessarily inconsistent with the DiD assumption. The econometrician can leverage plant fixed effects in a regression model to absorb the level differences in the outcome variable between the treatment and the control group.

We then revisit the test for the parallel pre-trends assumption. Appendix Figure A.1 plots the logged delivered coal prices by a plant's eventual divestiture status and a moderating variable. Panel (a) shows that average prices for treated plants were higher than for control plants with the outliers remaining in the sample. Nevertheless, the figure reveals no visual evidence for differential trends between the treatment and the control plants. In panels (b) and (c), the difference in the outcome variable between the subgroups of the treatment and the control plants also does

[24] Not excluding the outliers, the average treatment effect is 12% and statistically significant.

[25] We refer the reader to Han et al. (2020) for the details of ComEd's unique contract renegotiation history and different ways to account for the outliers.

Table A.1: Summary Statistics for Divested and Non-Divested Plants without Dropping ComEd Outliers

	(1)	(2)	(3)
	Divested	Not divested	Difference
Panel A: Time-invariant statistics as of 1997			
Plant vitage	1960.23	1960.15	0.08
	[11.48]	[15.27]	(2.85)
Pre-divestiture contract disadvantage	0.06	-0.04	0.10**
	[0.26]	[0.21]	(0.05)
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			
Log(Price ($/MMBtu))	0.39	0.29	0.10**
	[0.29]	[0.24]	(0.04)
Log(Price under contracts ($/MMBtu))	0.43	0.32	0.11**
	[0.30]	[0.25]	(0.04)
Millions MMBtu Delivered	3.61	3.15	0.46
	[3.55]	[3.23]	(0.57)
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 * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.2: Heterogeneous Effects of Divestiture on Log(Price) without Dropping ComEd Outliers

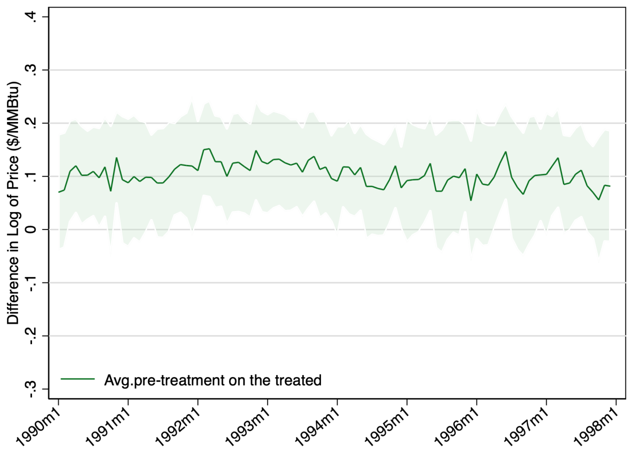
	(1)	(2)	(3)	(4)
$\mathbb{1}$(Divest)	$0.390^{**}_{\dagger\dagger}$	$-0.199^{***}_{\dagger}$	$0.668^{***}_{\dagger\dagger\dagger}$	0.001
	(0.159)	(0.054)	(0.175)	(0.046)
	[0.151]	[0.103]	[0.138]	[0.053]
$\mathbb{1}$(Divest)×(Incentive years)	$0.021^{***}_{\dagger\dagger}$	$0.036^{***}_{\dagger\dagger}$		
	(0.006)	(0.007)		
	[0.008]	[0.014]		
$\mathbb{1}$(Divest)×log(Capacity)	$-0.071^{***}_{\dagger\dagger\dagger}$		$-0.119^{***}_{\dagger\dagger\dagger}$	
	(0.022)		(0.027)	
	[0.018]		[0.027]	
$\mathbb{1}$(Divest)×Disadv.×$\mathbb{1}$(Disadv.>0)	$-0.798^{***}_{\dagger\dagger\dagger}$			$-0.951^{***}_{\dagger\dagger\dagger}$
	(0.175)			(0.173)
	[0.151]			[0.156]
$\mathbb{1}$(Divest)×Disadv.×$\mathbb{1}$(Disadv.<0)	-0.116			0.119
	(0.265)			(0.265)
	[0.289]			[0.267]
Avg. # of Matched Neighbors	6.5	6.5	6.5	6.5
Year-Month FE	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes
Plant-Level Clusters	206	206	206	206
Utility-Level Clusters	98	98	98	98
Divested Plants	87	87	87	87
Control Plants	119	119	119	119
R^2	0.765	0.732	0.728	0.758
Observations	38,093	38,093	38,093	38,093

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.

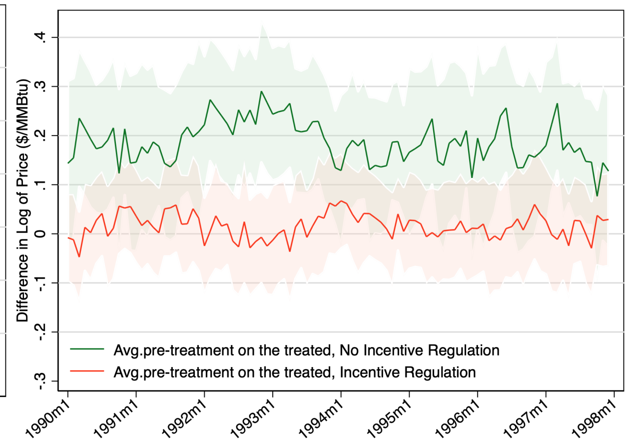
not seem to vary at a differential rate. In panel (d), however, the difference in log price seems to decline for the plants with disadvantaged contracts. Unlike in Figure 2, we include the ComEd plants in Figure A.1. This trend is consistent with Han et al. (2020) who document that ComEd's costs under their unfavorable contracts started to decrease well before divestiture. Nevertheless, we next show that the main results of the paper are robust to the presence of these outlier plants.

In Appendix Table A.2 we revisit the heterogeneous treatment effects in the main paper keeping the ComEd outliers in the sample. Though the coefficient estimates are similar to the counterparts in Table 3 across the board, the estimates on $\mathbb{1}$(Divest)×Disadv.×$\mathbb{1}$ (Disadv.>0) are noticeably higher. The estimates reflect the substantial cost reductions of the ComEd plants that were able to renegotiate their highly disadvantaged contracts before divestiture.

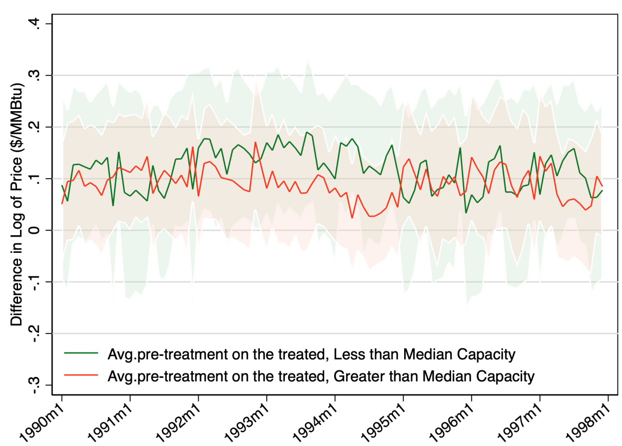
Figure A.1: Pre-Trends in Delivered Coal Prices without Dropping ComEd Outliers



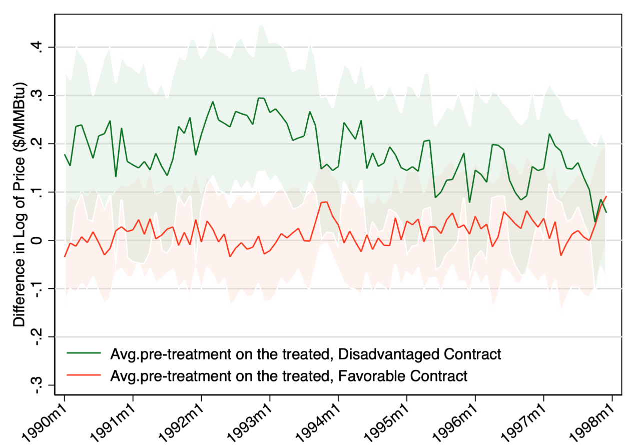
(a) Price Difference



(b) Price Difference by Incentive Regulation



(c) Price Difference by Median Capacity



(d) Price Difference by Contract Disadvantage

Notes: Panel (a) shows the difference in 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 costs by whether a plant is ever subject to incentive regulation in the pre-divestiture period. Panel (c) shows the difference in the weighted average of log costs by whether a plant's capacity in 1997 is greater than a median plant. Panel (d) shows the difference in the weighted average of log costs by whether a plant has disadvantaged or favorable contracts as of 1997. In all of the 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 ($\log(cost)_{jt} = \gamma_t + \gamma_t 1(Divest)_j + \epsilon_{jt}$) clustered at the plant level.

A.3 Robustness to Additional Matching

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, 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.

Appendix Table A.3 shows the summary statistics when we include pre-divestiture regulatory status in the set of matching criteria. While additional matching comes at a cost of losing 6 divested and 18 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 are 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.2 Panel (a) reveals no visual evidence of differential trends between the treatment and control groups with additional matching. Panels (c) and (d) also do not exhibit differential pre-trends between the treatment and control groups. In Panel (b), the difference between divested and non-divested plants seems to be slightly increasing for plants not subject to pre-period incentive regulation. We formally test whether the subgroups of the two groups based on pre-period incentive regulation had differential linear pre-trends.[26] We find that divested plants that were subject to incentive regulation were trending differentially at a statistically insignificant -0.04 percentage points per month from non-divested plants that were subject to incentive regulation (t-statistic of -1.24). Thus, we cannot reject the null hypothesis of equal pre-trends (neither for the average treatment effect nor for the different subgroups of divested and non-divested plants) using the standard pre-trend test.

Lastly, we find that the main results of the paper in Table 3 are not sensitive to the choice of including a plant's pre-divestiture incentive-regulation status in the set of matching variables. Appendix Table A.4 repeats Table 3 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.

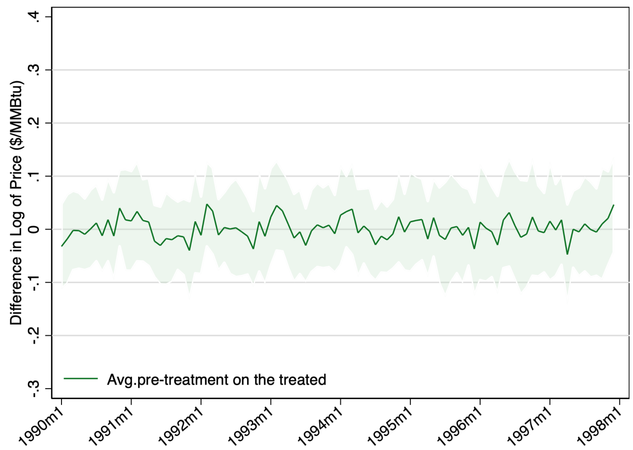
[26] We estimate a regression specification, $log(price)_{jt} = \delta_0 Time \times Moderator_{j,0} + \delta_1 Time \times \mathbb{1}(divest)_j \times Moderator_{j,0} + \tilde{\delta}_0 Time \times Moderator_{j,1} + \tilde{\delta}_1 Time \times \mathbb{1}(divest)_j \times Moderator_{j,1} + \gamma_j + \epsilon_{jt}$ where $Moderator_{j,1}$ is an indicator variable for whether a plant is subject to pre-divestiture incentive regulation and $Moderator_{j,0}$ takes the opposite value of $Moderator_{j,1}$. A hypothesis test for differential linear trends for different subgroups is to test whether $\tilde{\delta}_0$ and $\tilde{\delta}_1$ are different from 0, respectively.

Table A.3: Summary Statistics for Divested and Non-Divested Plants with Additional Matching

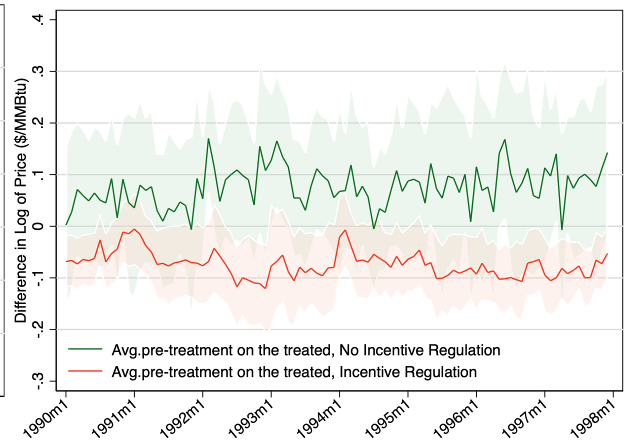
	(1)	(2)	(3)
	Divested	Not divested	Difference
Panel A: Time-invariant statistics as of 1997			
Plant vintage	1960.36	1960.23	0.13
	[12.12]	[15.33]	(3.77)
Pre-divestiture contract disadvantage	0.02	0.01	0.01
	[0.22]	[0.15]	(0.04)
Years of incentive regulation	2.14	3.63	-1.50
	[3.21]	[3.72]	(0.98)
Panel B: Monthly average statistics between 1990 and 1997			
Log(Price ($/MMBtu))	0.34	0.35	-0.01
	[0.21]	[0.19]	(0.03)
Log(Price under contracts ($/MMBtu))	0.38	0.37	0.00
	[0.21]	[0.19]	(0.03)
Millions MMBtu Delivered	3.82	2.97	0.85
	[3.61]	[3.03]	(0.65)
Percent bituminous	0.86	0.84	0.02
	[0.34]	[0.36]	(0.08)
Percent sub-bituminous	0.05	0.07	-0.02
	[0.21]	[0.24]	(0.03)
Heat content (MMBtu/ton)	23.38	23.25	0.13
	[3.91]	[4.11]	(1.02)
Sulfur content (lbs/MMBtu)	1.59	1.47	0.12
	[0.80]	[0.97]	(0.18)
Ash content (lbs/MMBtu)	10.42	10.14	0.28
	[6.25]	[7.95]	(1.52)
Distance to mine (mi.)	203.40	243.65	-40.25
	[234.67]	[241.40]	(39.83)
Annual capacity (MW)	1006.26	907.78	98.48
	[776.63]	[744.19]	(184.51)
Annual capacity factor	0.53	0.47	0.06
	[0.18]	[0.20]	(0.05)
Plants	74	87	161

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.

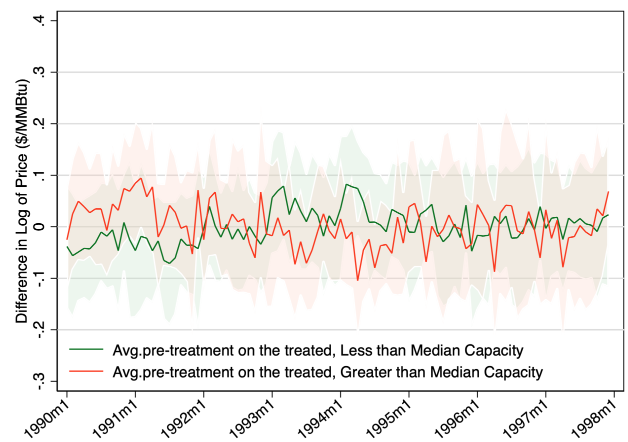
Figure A.2: Pre-Trends in Delivered Coal Prices with Additional Matching



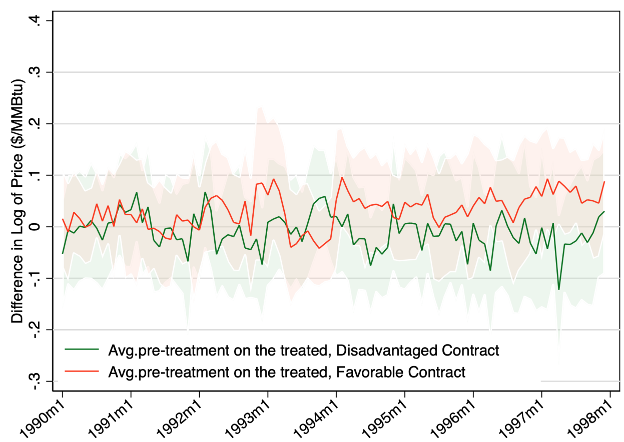
(a) Price Difference



(b) Price Difference by Incentive Regulation



(c) Price Difference by Median Capacity



(d) Price Difference by Contract Disadvantage

Notes: Panel (a) shows the difference in 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 costs by whether a plant is ever subject to incentive regulation in the pre-divestiture period. Panel (c) shows the difference in the weighted average of log costs by whether a plant's capacity in 1997 is greater than a median plant. Panel (d) shows the difference in the weighted average of log costs by whether a plant has disadvantaged or favorable contracts as of 1997. In all of the 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 ($\log(cost)_{jt} = \gamma_t + \gamma_t 1(Divest)_j + \epsilon_{jt}$) clustered at the plant level.

Table A.4: 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 using plant-level clustering * p<0.10,** p<0.05, *** p<0.01. Daggers denote statistical significance using utility-level clustering † p<0.10,†† p<0.05, ††† p<0.01.

A.4 Alternative Specification for Heterogeneous Treatment Effects

In Appendix Table A.5, we show the robustness of the heterogeneous treatment effects presented in Section 4.3 with an alternative regression specification. Instead of estimating Equation (3), we estimate the following regression specification:

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

where $\mathbb{1}(ever\ incentive)_j$ 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 plant j is above the median capacity as of 1997. $\mathbb{1}(disadv. > 0)_j$ is an indicator variable for whether plant j's contract is disadvantaged. Note that in the regression specification in the main paper (Equation (3)), we interacted this term with a linear disadvantage term (i.e., $(disadv.)_j \times \mathbb{1}(disadv. > 0)_j$). By classifying the divested plants into two simple groups for each heterogeneous effect, we test for non-linearity in the heterogeneous effects (i.e., whether a few influential observations drive the results obtained from estimating the preferred specification in Section 4.3). 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.5: Alternative Specification for Heterogeneous Effects of Divestiture on Log(Price)

	(1)	(2)	(3)	(4)
$\mathbb{1}$(Divest)	-0.056	$-0.139^{***}_{\dagger\dagger}$	0.003	-0.022
	(0.050)	(0.048)	(0.042)	(0.040)
	[0.057]	[0.057]	[0.047]	[0.043]
$\mathbb{1}$(Divest)×(Incentive ever)	$0.115^{***}_{\dagger\dagger}$	$0.137^{***}_{\dagger\dagger\dagger}$		
	(0.041)	(0.043)		
	[0.052]	[0.052]		
$\mathbb{1}$(Divest)×$\mathbb{1}$(Capacity>Median)	$-0.089^{**}_{\dagger\dagger}$		$-0.117^{***}_{\dagger\dagger\dagger}$	
	(0.041)		(0.043)	
	[0.040]		[0.040]	
$\mathbb{1}$(Divest)×$\mathbb{1}$(Disadv.>0)	-0.066			$-0.098^{**}_{\dagger}$
	(0.041)			(0.045)
	[0.047]			[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 using plant-level clustering * p<0.10,** p<0.05, *** p<0.01. Daggers denote statistical significance using utility-level clustering † p<0.10,†† p<0.05, ††† p<0.01.