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Essays On The U.S. Electricity Sector

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Abstract

This dissertation consists of two essays on the U.S. electricity sector.

The first essay studies the impact of electricity market deregulation on a firm's fuel procurement costs. I find that deregulated coal-fired power plants achieve about 6% cost reduction, half of what literature has claimed. Furthermore, when the Acid Rain Program, environmental regulation on sulfur dioxide, induces deregulated plants to disproportionately switch to cleaner and cheaper sub-bituminous coal, it is challenging to identify what cost reductions would have been absent the environmental regulation. I estimate 3\% as the lower bound effect of deregulation. Despite the small effect on average, plants exhibit heterogenous reponses to deregulation. When plants procure coal via bilateral contracts, an amount of cost reductions a plant can attain depends on its incentive and ability to negotiate. I find that deregulated plants with unfavorable contracts and bigger production capacity achieve substantial cost reductions; 18% and 9%, respectively.

The second essay studies the impact of power company mergers on fuel sourcing decisions. Specifically, I study whether coal-fired power plants consolidate their supplier base upon a merger. I find that a pair of two merging plants becomes 23% more likely to source from common suppliers. Plant pairs also increase their dependence on the common suppliers. I find that a fraction of total coal delivery from the common suppliers increases by 47% upon a merger.

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To

the family

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ABSTRACT

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Jin Soo Han

Jean-François Houde

This dissertation consists of two essays on the U.S. electricity sector.

The first essay studies the impact of electricity market deregulation on a firm's fuel procurement costs. I find that deregulated coal-fired power plants achieve about 6% cost reduction, half of what literature has claimed. Furthermore, when the Acid Rain Program, environmental regulation on sulfur dioxide, induces deregulated plants to disproportionately switch to cleaner and cheaper sub-bituminous coal, it is challenging to identify what cost reductions would have been absent the environmental regulation. I estimate 3% as the lower bound effect of deregulation. Despite the small effect on average, plants exhibit heterogenous reponses to deregulation. When plants procure coal via bilateral contracts, an amount of cost reductions a plant can attain depends on its incentive and ability to negotiate. I find that deregulated plants with unfavorable contracts and bigger production capacity achieve substantial cost reductions; 18% and 9%, respectively.

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[1] Joint with Jose Miguel Abito, Jean-François Houde, Nathan Miller, and Matthew Weinberg

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Chapter 1

Deregulation and Input Costs: Revisiting the Case of the U.S. Electricity Market

1.1 Introduction

In the late 1990's, the U.S electricity industry was sailing through a storm of regulatory changes. Cost-of-service regulation was repealed in many states based on an intellectual premise that market competition improves efficiency. One of the ideas behind the promised efficiency gain was that plants would face increased incentives to reduce their input costs once they become residual claimants to cost savings in a deregulated market.[2] However, the cost-minimization incentives were not completely absent under the traditional regulation.[3] Many state regulators had modified their fuel cost pass-through programs such that firms "absorb a portion

[2]Under cost-of-service regulation, regulated utilities recover their fuel expenditure as long as state regulators approve the expenditure to be prudent.

[3]See Abito (2016) for a survey of other incentives that utilities have under rate-of-return regulation.

of fuel cost overruns as well as profit from lower than expected fuel costs" (Knittel, 2002*a*). To that end, the effectiveness of deregulation has remained as an empirical question until Cicala (2015) has provided the first evidence that deregulation leads to, on average, 12% fuel cost reductions.

However, the average effect is nuanced in that it can mask the general efficacy of treatment when the distribution of the effect is skewed. When power plants typically procure coal via bilateral contracts, it is unlikely that all the plants are able to leverage deregulation to achieve unilateral cost reductions. In this paper, I show that deregulation indeed has a heterogeneous effect across different plants depending on their incentive and ability to negotiate contracts.

But first, I replicate Cicala's finding and show that his results are heavily influenced by a subset of the plants treated by an event unrelated to deregulation. I employ the same matched difference-in-difference estimation where power plants that are divested by regulated utilities (as a mandatory compliance for deregulation) are compared to the plants that never become divested. The key assumption of the methodology is that divested plants would not have behaved differentially from non-divested plants absent the treatment. The fuel costs delivered to the plants owned by Commonwealth Edison Co. (7 of the 88 divested plants), however, started to decline in the early 90's prior to their divestitures in the late 90's. Albeit an effort to reduce costs, the company's contract re-negotiation was not a response to increase cost competitiveness in a deregulated market.[4] Nonetheless, the adjusted contract continued to lower delivered coal prices to the company's plants post divestiture. Then, including the ComEd plants in the econometric analysis inflates the average treatment effect. When rather the anomalous plants are ex-

[4]Commonwealth Edison's re-negotiation predates Illinois market deregulation by about five years.

cluded, the average effect becomes 6% and statistically indistinguishable from 0.

I then claim that the revised effect is further confounded by an environmental regulation, the Acid Rain Program (ARP). The introduction of an emissions cap on sulfur dioxides (SO_2) has incentivized regulated plants to comply by installing capital-intensive abatement equipment (also known as the Averch-Johnson effect (1962)). In comparison, divested plants have been much more likely to switch to low-sulfur sub-bituminous coal. Unfortunately, sub-bituminous coal have been not only cleaner but also cheaper. By switching coal types, divested plants could not only comply with the environmental regulation but also earn a windfall drop in fuel costs. However, because the implementation of the ARP coincides with that of market deregulation, it is hard to separately identify cost reductions due to respective policy changes.

Conditional on burning the same type of coal, divestiture leads to about 3% cost reductions. The rest of the total 6% cost reductions is then attributed to coal-switching. This does not grant that the ARP alone explains all the fuel-switching. Absent the market deregulation, more plants would have remained rate-regulated and therefore would have adopted scrubbers, resulting in less coal-switching. However, vice versa is also true: absent the environmental regulation, divested plants would have switched less. Focusing on the plants that had installed scrubbers before the ARP and hence face minimal levels of compliance, I find that divested plants switch coal types just as much as non-divested plants. Depending on the extent to which the ARP influences coal-switching, the average effect of divestiture would range from 3%˜6%.

The overall insignificant effect of divestiture, however, does not mean that deregulation was a failure altogether. When 81 divested plants spend about $5 billion annually, 3%˜6% cost reductions are economically important numbers as they translate into \$150~\$300 million annual savings. Moreover, deregulation was effective for certain type of plants. In fact, when power plants procure coal via bilateral long-term contracts, an amount of cost reductions a plant can realize hinges on its incentive and/or ability to negotiate the contract terms. First, I observe that plants with contracts that charge about the market price would not gain much from renegotiation. In contrast, plants stuck in unfavorably high cost contracts could seize divestiture as an opportunity to bargain better terms (Joskow, 1988). Empirically, cost reductions for divested plants with the pre-treatment contract costs one standard deviation (38%) higher than their neighbors' are 21 percentage point more than those for the divested plants with no such cost disadvantage. Second, literature on countervailing power predicts that bigger buyers often extract larger concessions from suppliers.[5] I find that cost reductions for divested plants with production capacity one-standard-deviation (730 megawatts) bigger than the average are 12 percentage point more than those for the divested plants with the average capacity. Heterogeneity in the treatment effect suggests that successful deregulation does not depend solely on a plant's cost-minimization incentives. Rather, it requires an understanding of an intricate interaction between the power producers and the coal suppliers.

The rest of the paper proceeds in the following order. Section 1.2 briefly documents the history of electricity market deregulation in the U.S., and explains why plant divestitures take place in certain states but not in others, providing a natural experiment research design. In Section 1.3, I explain the research design and test the validity of the estimation strategy in the data. In Section 1.4, I revisit and discuss Cicala's findings. Section 1.5 documents the role of the Clean Air Act Amend-

[5]See Section 1.6 for theoretical explanations that a plant's size is associated with better bargaining leverage.

ments in explaining the cost reductions by the divested plants. In Section 1.6, I explore heterogeneity in the treatment effect and provide evidence that deregulation was effective for certain type of plants.

Related Literature

This paper contributes to two bodies of literature. First, the paper contributes to the empirical literature on the consequences of the U.S. electricity market deregulation.[6] An important departure of my paper is that I consider a role of bilateral contracts in explaining different plants' heterogeneous responses to the market deregulation.

Researchers have so far examined deregulation's impact on the wholesale market performance (Borenstein, Bushnell and Wolak, 2002; Joskow and Kahn, 2002; Bushnell, Mansur and Saravia, 2008; Hortacsu and Puller, 2008), emissions compliance on SO_2 (Fowlie, 2010), consumer choices in retail competition (Hortacsu, Madanizadeh and Puller, 2012), investment decisions by plant operators (Ishii and Yan, 2002), and safety of nuclear power plants (Hausman, 2014). This paper closely relates to studies that examine the aspects of operating performance (i.e., fuel efficiency, plant utilization rates, non-fuel operating expenses, etc.) of the power plants (Kleit and Terrell, 2001; Wolfram, 2005; Douglas, 2006; Fabrizio, Rose and Wolfram, 2007; Zhang, 2007; Davis and Wolfram, 2012; Chan et al., 2013; Craig and Savage, 2013). Yet, unlike the previous studies, this paper focuses its attention to the largest portion of the operating expenses, fuel procurement costs. Though Cicala (2015) is the first to provide an evidence of fuel cost reductions from market deregulation, I further explore how a plant's incentive and ability to negotiate its

[6] Researchers have examined electricity market deregulation in other countries as well. Selected papers include Newbery and Pollitt (1997); Wolfram (1999); Cropper et al. (2011).

procurement contracts shape its response to market deregulation.

Second, the paper touches upon the empirical bargaining literature on how a firm size affects negotiated prices between the firm and its suppliers (i.e., countervailing power literature). Since Galbraith (1954) introduced the concept of countervailing power, a number of theoretical papers have predicted that an agent of a bigger size can extract more concessions from its bargaining counter-party (Horn and Wolinsky, 1988; Stole and Zwiebel, 1996; Chipty and Snyder, 1999). However, empirical studies have been mostly limited to applications in the health care industry (Sorensen, 2003; Ho, 2009; Ellison and Snyder, 2010; Lakdawalla and Yin, 2015). This paper brings the theoretical prediction of countervailing power to the transactions between power plants and coal mines and shows that when deregulation provides plants an opportunity to re-negotiate, a plant size matters in negotiating a lower price with the suppliers.

1.2 Institutional Background

In order to examine the impact of deregulation on fuel costs, this study relies on a natural experiment in the U.S electricity sector that took place in the late 1990's.[7] Historically, a vertically integrated power utility owned and operated production and distribution of electricity in a regional market. A state regulator often imposed rate-of-return regulation to these local monopolies in order to protect consumers from the potential abuse of market power. The rate-based regulation typically sets a rate at which power utilities earn profits based on capital, and reimburses any prudent operating costs (hence the other name, cost-of-service regulation). Yet over time, the efficiency of cost-of-service regulation and local monopolies was

[7] See Joskow (1997) and Griffin and Puller (2009) for the details of the history of the U.S electricity sector deregulation.

called into question as local electricity markets could be integrated over a greater region.

In the mid 90's, the federal government started an initiative to make the electricity sector more competitive.[8] The idea was to introduce competition in the generation segment of the electricity sector by splitting generation from transmission and distribution. Although actual implementation was up to states' discretion, most states seemed interested in the idea of deregulation. All states at least considered the prospects by 1998 (Fabrizio, Rose and Wolfram, 2007).

The deregulation momentum, however, dissipated quickly in the summer of 2000 as the California electricity crisis broke out. Many states stopped considering deregulation, and a handful that had already begun the restructuring process suspended further action. Meanwhile, the states that had already finished or that had made a far enough progress in the process did not reverse back to the regulation (Griffin and Puller, 2009; Borenstein, 2002). In such states, generation facilities of regulated utilities were required to be divested.[9] Once divested, a plant's operating costs were no longer subject to rate regulation as it competed in a deregulated wholesale market. Plants in all other non-deregulated states remained under the status quo cost-of-service regulation. Many researchers have exploited this natural experiment that some plants undergo divestiture while others remain regulated as an empirical research design (Bushnell and Wolfram, 2005; Wolfram, 2005; Dou-

[8] 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, the FERC issued Order No. 636, known as the Restructuring Rule, which mandated open access to the transmission system and separation of electricity sales from transportation services. The FERC also mandated non-discriminatory pricing and access to transmission services –Order No.888 in 1996–, and established legal grounds for voluntary, non-profit organizations –Order No. 2000 in 1999– that would manage the wholesale electricity markets.

[9] Utilities were allowed to transfer their generation assets to unregulated affiliates. That is, one umbrella parent company could own both the generation facilities and the regulated transmission/distribution facilities. But, the generation facilities had to operate independently from the transmission/distribution facilities.

glas, 2006; Fabrizio, Rose and Wolfram, 2007; Davis and Wolfram, 2012; Chan et al., 2013; Craig and Savage, 2013; Hausman, 2014; Cicala, 2015). This paper adopts a similar strategy and explores the differences in fuel procurement costs between the divested plants and the non-divested plants.[10]

However, researchers have also cautioned against a potential selection bias of this estimation strategy. States with high electricity prices were more likely to deregulate (White, Joskow and Hausman, 1996). If the high prices were driven by expensive coal procurement costs, one could argue that cost reductions by high-cost deregulated plants were simply the mean reversions. Fortunately, the high electricity prices were mostly due to the high construction costs of nuclear plants (White, Joskow and Hausman, 1996; Davis and Wolfram, 2012; Chan et al., 2013). Rate-of-return regulation incorporated construction costs in the electricity prices as the prices were set based on the capital investment. Although it is impossible to completely eliminate the identification concern, there is no evidence, at least to my knowledge, that restructuring decisions of the states depended on the performance of coal-fired power plants.

1.3 Methodology & Data

1.3.1 Matched Difference-in-Difference Estimator

In this paper, I employ the same matched differences-in-differences (DiD) estimator as in Cicala (2015).[11] The treatment group consists of divested plants in deregulated states while the control group consists of regulated plants that remain under

[10] Technically, the set of regulated plants also includes plants that are never subject to divestiture as they are owned by the government, municipalities or local cooperative organizations.

[11] The matched DiD approach is similar to Heckman et al. (1998). Matching is implemented on the actual characteristics of facilities rather than the propensity score as in Cicala.

the status quo cost-of-service regulation. The outcome variable is overall delivered coal prices, which represent the total costs of fuel procurement and shipping. However, the econometrician does not separately observe the individual components of the delivered coal prices while she wants to estimate the impact of deregulation only on the procurement costs. Then, a classic endogeneity problem arises. Matching plants in close proximity (proxy for shipping destination) that burn the same type of coal (proxy for shipping origin) solves the problem. It allows to compare plants that share the same shipping costs and more generally the same fuel procurement environment such as the number of potential coal suppliers to a region.[12]

In practice, two criteria are used for matching: 1) geographic proximity of and 2) the type (rank) of coal burned at the plants. First, the geographic proximity is defined to be either the m closest neighbors or plants within the caliper distance d. A number of choices (i.e., 10, 5, 1 nearest neighbors and the control plants within 200, 100, 50 miles) are tested but the 200 mile distance threshold and the 10 nearest neighbor specification are reported as a main specification throughout the paper. The majority type of coal burned at the plant in 1997 is used for the definition of plant's coal type. Coal type can be bituminous, sub-bituminous or other. The "other" category represents less than 3% of the data.[13]

[12]It is true that endogenous shipping costs become less of a problem if they are time-invariant for each plant or time-varying but uniformly across plants such that plant or time fixed effects can absorb the variation in the delivered prices due to shipping. But, the 'Coal Transportation: Rates and Trends' by the EIA (2004) documents that shipping costs of coal from a particular basin to different regions of the country have varied differentially over time. Busse and Keohane (2007) also show that shipping costs of sub-bituminous coal from the Powder River Basin varies across regions over time. Then, unless divested plants and non-divested plants are located nearby and use the same type of coal, it becomes infeasible to determine whether the post-treatment differences in the coal prices between the two comparison groups reflect the pre-existing differences in the shipping costs or the actual differences in the fuel procurement costs.

[13]The theoretical problem with the plant type definition based on 1997 is that the definition categorizes plants that switch their coal type in different years into the same plant type. Because of the Phase I of the Clean Air Act, some plants switch from bituminous to cleaner sub-bituminous

Given the working definitions of matching criteria, a matched DiD is carried out in two stages. The first stage identifies a set of matches from the control group according to the matching criteria, which in our setting are proximity and rank of coal burned at the plant. In the second stage, weighted averages of the outcome variable are constructed by the treatment and the control group before and after treatment. A DiD estimator is then calculated in a standard way. This matched DiD can also be carried out in a regression setting with appropriate matching criteria and weights.

Formal definitions of the matching criteria and the estimation equation are denoted in the following way. Let i denote a plant and t denote the time period. Let K be the set of all the potential control facilities (i.e. a set of plants minus the divested plants). First, a set of plants is identified as the matched control group. For a particular treated facility k, a set of control plant j's that satisfies

$$\sum_{j \in K} 1(||dist_{kj}|| \leq ||dist_{kj'}||) = m$$

$$||dist_{kj}|| < d$$

where $||dist_{kj}||$ denotes the distance between j and k, respectively identifies the m nearest plants and the plants within the caliper distance d of the plant k. The equation $rank_j = rank_k$ identifies the control plants that burn the same rank of coal as the treated plant k. Given the set of treatment plants and the matched counterpart, the average treatment effect on the treated can be estimated by the

coal in 1995. While these plants are categorized as the sub-bituminous plant, they exhibit differential trends in fuel costs from the plants that always burn sub-bituminous coal in the pre-treatment period. To that end, I adjust the definition by assigning plants to a particular rank only if the same rank coal is burned in all the years from 1990 to 1997. Plants that switch their coal type in any year before 1997 are classified into the "other" category. Empirically, this leads to minor changes in the estimated coefficients and does not overturn the results in Cicala.

following regression equation:

$$log(p_{it}) = \beta \mathbf{1}(divest)_{it} + \gamma_i + \delta_t + \epsilon_{it} \tag{1.3.1}$$

where p_{it} denotes the delivered coal prices, γ_i denotes the plant fixed effects, δ_t denotes the time (year-month) fixed effects, and $\mathbf{1}(divest)_{it}$ denotes a dummy equal to 1 if the plant i is treated and t is after the treatment period. Given Equation 1.3.1, the matched DiD estimator can be estimated if each matched control plant is weighted by the inverse of the number of matches to a treated plant. Intuitively, for each treated plant, the inverse weighting constructs one synthetic control plant from the pool of the matched control plants. Note that, Equation 1.3.1 is an unweighted unmatched OLS DiD estimation if one drops the first stage matching and the weighting procedure. If one allows time fixed effects to differ flexibly across regions, the OLS DiD could also account for the unobserved time-varying differences across regions.[14]

1.3.2 Data

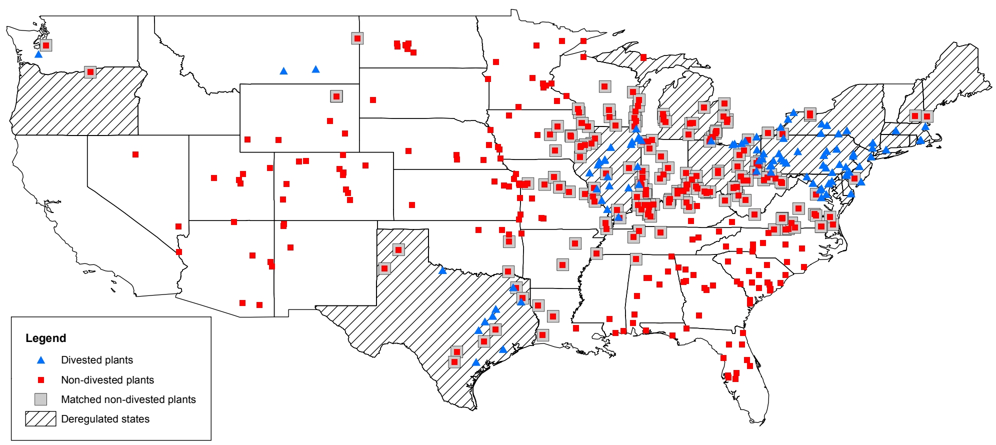
Comprehensive data used in this paper is constructed by merging three databases from the Energy Information Administration (EIA). EIA's form 923, form 860 and form 906 provide information on a plant's fuel expenditure, characteristics, and

[14]It is important to keep in mind that subjective matching criteria assign somewhat arbitrary weights to the matched control plants. In general, the matched DiD estimation has the weights sum to 2*(# of divested plants) in each period t where t is typically 0 or 1. For each treated plant, a number of matched control plants are averaged to construct one synthetic counterpart. In other words, from the perspective of control plants, a greater weight is assigned to the plant if it is matched to multiple divested plants. That is, a control plant receives different weights depending on which matching criteria are imposed. Although the empirical results are not sensitive to the choice of cutoff, matching does load greater weights to a particular set of control plants. Because one might worry that the estimation results are cherry-picked from over-weighting certain control plants, the unmatched, unweighted (OLS) DiD estimation is performed to confirm the matched DiD results. See Abadie, Diamond and Hainmueller (2012) for detailed explanations on the weighting procedure.

divestiture status, respectively. One critical caveat of the data is that a part of the fuel expenditure data is missing for the divested plants. Regulated plants upon divestiture were no longer required to report until 2002 when the EIA resumed collecting the data. 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 the DiD estimator. The estimator measures the differences in the outcome variable before and two-year-after the treatment as opposed to before and after. The short-term effects of less than two years can only be identified from a set of plants that become divested post 2002.

The locations of coal-fired power plants in the U.S. are plotted in Figure 1.3.1 by the eventual divestiture status. Note that most of the Northeast states are deregulated and the number of regulated plants in the region is small. The analysis includes fewer plants as one imposes more strict matching criterion on geographical proximity.

Figure 1.3.1: Coal-fired Plants in the United States, 1990-2009



Notes: Plants that exit before 1997, enter after 2002, and cease reporting after 2002 are not shown on the map. Matching criterion: d = 200 mi.

1.3.3 Validity of the DiD estimator: Graphical Analysis

Since the DiD estimation relies on comparing changes between the treatment and the control plants, it is important to examine whether the control plants could serve as a reasonable counterfactual for the treatment plants. Table 1.1 summarizes the number of total and matched non-divested plants according to various matching criteria.

Table 1.1: Plant Typology

	Divested	Matched non-divested*	Matched non-divested**	Total non-divested
Bituminous	66	57	108	190
Sub-bituminous	10	16	16	69
Other	12	13	13	48
Total	88	86	137	305

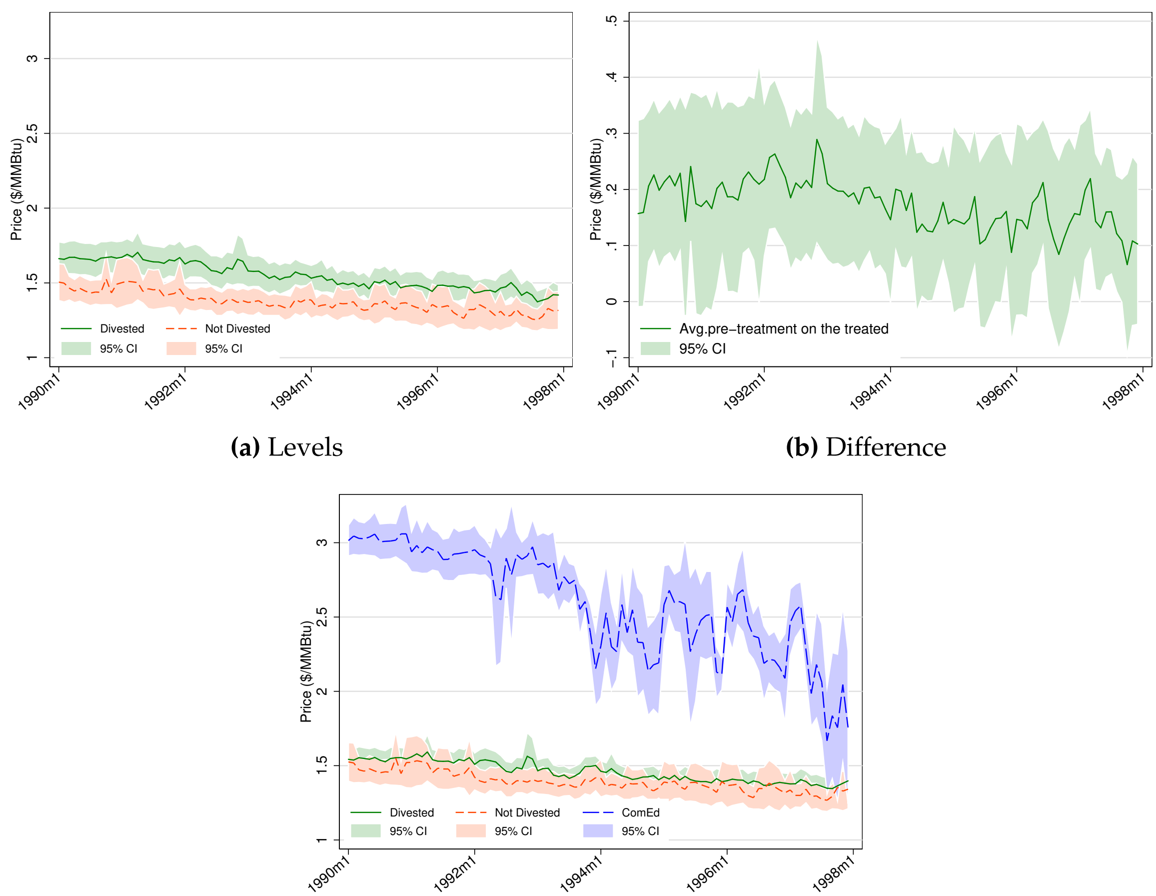
Notes: * indicates the 10 nearest neighbor matching. ** indicates the 200 mi. caliper distance matching. Coal rank is based on the years from 1990 to 1997.

Given the matched non-divested plants, I compare divested plants and their matched counterparts for 1) parallel pre-trends of the outcome variable and 2) a balance in the observable characteristics in the pre-period. Panel (a) of Figure 1.3.2 shows the pre-trends between the divested and the matched non-divested plants. Panel (b) of Figure 1.3.2 emphasizes the difference in the delivered coal prices between the two comparison groups. Although the pre-trends seem somewhat parallel at the first glance, a closer look at the difference reveals that there is a slight narrowing of the gap around 1993. Formally, I perform two hypothesis tests with two respective nulls, $H_0 : \delta_{\text{divested plants}} = \delta_{\text{matched non-divested plants}}$ and $H_0 : \gamma_{\text{divested plants}} = \gamma_{\text{matched non-divested plants}}$, where δ and γ are defined in the following equation:

$$log(p_{it}) = \gamma_l + \delta_l t + \epsilon_{it}$$

t indicates a monthly time variable from January 1990 to December 1997, i denotes a plant, and l denotes either a group of divested plants or that of non-divested plants. Intuitively, δ_l and γ_l are a linear time trend and the average coal costs in log for the group l. The Wald tests on the respective null hypotheses provide p-values of 0.042 and 0.011, implying that at the 5% significant level the pre-trend and the mean fuel costs of divested plants are different from those of the non-divested plants.[15] Though one might argue that the 5% significance level is insufficient to rule out the possibility of parallel pre-trends, it is important to remember that the analysis is performed on the matched sample. Differential averages and trends of the delivered costs at the two comparison groups imply that divested plants and nearby non-divested plants differ fundamentally beyond the shipping costs.

Figure 1.3.2: Pre-trends in Delivered Coal Prices



(a) Levels

(b) Difference

(c) Levels, ComEd partitioned

Notes: The 10 nearest neighbor matching is used. Coal rank is based on the years from 1990 to 1997. Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Confidence intervals are based on standard errors clustered at the plant level.

[15] The p-values in the text are estimated using the 200 mi distance caliper distance matching. The 10 nearest neighbor matching yields the p-values of 0.059 and 0.016 for testing the differences in the time trend and the mean fuel costs, respectively.

1.3.4 Validity as Treatment Group: 7 ComEd Plants in Chicago

In this subsection, I document that the fuel cost reductions by the seven Commonwealth Edison Co. (ComEd) plants were triggered by re-negotiation of old contracts but not by divestiture. Once the ComEd plants are excluded from the sample, the parallel pre-trends are restored, the average coal prices at the divested plants are no longer different from those at the non-divested plants.

The story of ComEd's contract re-negotiation dates back to the 1970's. Beginning in the 70's, Chicago mandated that power utilities burn cleaner coal as part of compliance with the Clean Air Act (the very initial one enacted in 1973). Like other utilities, ComEd substituted away from high-sulfur bituminous coal to low-sulfur sub-bituminous coal produced in Montana and Wyoming. It entered into over 30-year contracts that were to last until the early 2000's. The terms of contracts were such that the delivered coal prices were to gradually escalate over time based on a predetermined rate.[16] The contracts were reasonable given the energy crisis in the 70's. But in the following decade, the coal sector grew remarkably in Montana and Wyoming, and by the mid 80's, sub-bituminous coal from the region had become substantially cheaper.

Facing much more competitive market prices, many utilities that had entered into long-term contracts in the 70's either re-negotiated or bought out their old contracts in the 80's. However, ComEd failed to do so. After several failed attempts to buy out its old contracts, the company tried to cut the deliveries from what it had contracted for. As ComEd lost a lawsuit filed by the coal suppliers for breaching the contracts, it had to resume its expensive coal deliveries. By the early 90's, ComEd was paying about twice above the market price for its coal. In 1992, the

[16] See Decker Coal Co. v Commonwealth Edison Co., 805 F.2d 834, Decker Coal Co. v Department of Revenue of the State of Montana & The State Tax Appeal Board of the State of Montana, No. 99-078. and the 10-K form of ComEd (1993) for the detailed history of ComEd's coal contracts.

City of Chicago, the state's attorney's office and the state's Office of Public Counsel filed an official complaint to the Illinois Commerce Commission (abbr., ICC, i.e., Illinois Public Utility Commission) to audit ComEd's rate case. Pressured by accentuated media coverage, ComEd soon reached a settlement with the ICC for refunds and re-negotiated its coal contracts before the official ruling of the ICC.[17] The renegotiated terms promised lower rates but ComEd also had to take a bullet. The reduced coal prices were still above the market price, the reduction itself was gradual over time, and the expiration dates had to be extended to the late 2000's to the early 2010's (ComEd, 1993).

Panel (c) of Figure 1.3.2 shows that, though noisy, the delivered coal prices at the ComEd plants started to trend differentially in early 1993. It also shows that the prices at the ComEd plants were much higher than those at other plants. Hence, when the ComEd plants are excluded from the sample of divested plants, not only the parallel pre-trend assumption is reinstated but also the gap in the pre-trends (shown in Figure 1.3.2) is removed. Formally, for the parallel pre-trend assumption, I repeat the hypothesis tests in Subsection 1.3.2: $H_0 : \delta_{\text{divested plants}} = \delta_{\text{matched non-divested plants}}$ and $H_0 : \gamma_{\text{divested plants}} = \gamma_{\text{matched non-divested plants}}$, where δ and γ represent a linear time trend and the mean fuel costs. The p-values from the respective hypothesis tests are 0.448 and 0.292, implying that the differences in the pre-trends and the average fuel costs of the two comparison groups are stabilized.[18] This is because differentially decreasing delivered coal prices at the ComEd plants previously caused the linear time trend of the divested plants to be more negative than that of the non-divested plants. Figure 1.3.2 also confirms that

[17]See Karwath (1991; 1993), Maclean(1992*c*; 1992*a*; 1992*b*), and Boyd (1993) for more detailed story of how ComEd was pressured to re-negotiate.

[18]The p-values in the text are estimated using the 200 mi distance caliper distance matching. The 10 nearest neighbor matching yields the p-values of 0.502 and 0.336 for testing the differences in the time trend and the mean fuel costs, respectively.

ComEd's coal prices decrease gradually over time.

1.3.5 Summary Statistics

With the ComEd plants excluded, matching yields a great balance in the observable characteristics between the divested and the non-divested plants. Table 1.2 presents summary statistics of various plant characteristics in 1997, a year before the first divestiture. For completeness, I provide the statistics of both the full sample and the matched sample. In the full sample panel without matching, many non-divested plants in the West and the Southeast (non-divested plants without the gray coloring) are included. Because these plants tend to use sub-bituminous coal from Wyoming and Montana, their average prices, bituminous percent, and sulfur content of the total coal deliveries are lower, and delivery distance is greater than those at the divested plants. Matching allows to compare plants with similar shipping conditions and the same coal type, eliminating the mean differences in the observables between the two comparison groups.

Table 1.2: Characteristics of Divested and Non-divested Plants in 1997

	Full sample			Matched only		
	Divested	Not divested	Difference of means	Divested	Not divested	Difference of means
Capacity (MW)	939.11	881.06	58.05	939.11	749.13	189.98
	[757.81]	[752.68]	(94.86)	[757.81]	[712.72]	(145.13)
Plant Vintage	1959.83	1963.67	-3.84**	1959.83	1959.11	0.71
	[12.26]	[14.49]	(1.60)	[12.26]	[14.50]	(2.85)
Annual capacity factor	0.53	0.55	-0.02	0.53	0.49	0.04
	[0.19]	[0.20]	(0.02)	[0.19]	[0.25]	(0.06)
scrubbed	0.26	0.32	-0.06	0.26	0.24	0.02
	[0.44]	[0.47]	(0.06)	[0.44]	[0.43]	(0.08)
Millions MMBtu Delivered	44.68	45.24	-0.55	44.68	35.90	8.78
	[43.90]	[43.49]	(5.49)	[43.90]	[38.05]	(7.74)
Price ($/MMBtu)	1.37	1.23	0.14***	1.37	1.34	0.03
	[0.29]	[0.35]	(0.04)	[0.29]	[0.31]	(0.07)
Percent spot market	0.26	0.27	-0.01	0.26	0.29	-0.03
	[0.31]	[0.32]	(0.04)	[0.31]	[0.37]	(0.07)
Years remaining on contracts	4.15	5.11	-0.96	4.15	5.41	-1.26
	[6.27]	[6.04]	(0.89)	[6.27]	[6.25]	(1.35)
Percent in-state	0.46	0.34	0.12**	0.46	0.38	0.08
	[0.46]	[0.45]	(0.06)	[0.46]	[0.45]	(0.09)
Percent bituminous	0.83	0.65	0.18***	0.83	0.82	0.01
	[0.36]	[0.45]	(0.05)	[0.36]	[0.37]	(0.07)
Sulfur content (lbs/MMBtu)	1.40	1.17	0.23**	1.40	1.45	-0.05
	[0.78]	[0.90]	(0.10)	[0.78]	[0.89]	(0.16)
Ash content (lbs/MMBtu)	9.89	9.14	0.75	9.89	9.81	0.08
	[5.31]	[4.51]	(0.64)	[5.31]	[6.77]	(1.13)
Distance to mine (mi.)	269.33	350.34	-81.01**	269.33	268.34	0.99
	[290.05]	[315.25]	(37.13)	[290.05]	[305.39]	(52.23)
Plants	81	291	372	81	78	159

Notes: The 10 nearest neighbor matching is used. Coal rank is based on the years from 1990 to 1997. Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Brackets indicate standard devisions. Parentheses indicate standard errors which are clustered at the plant level. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level.

1.4 Replication and Revised Diff-in-Diff Estimation

1.4.1 Replication

Given the access to the EIA's restricted fuel delivery data, I independently replicate and confirm the results in Cicala (2015). Table 1.3 reports the replication results.

Same results can be found in Table 2 and Table 3 of Cicala's paper.[19] The first specification reports the unmatched, unweighted OLS DiD estimation results with the plant and the division-year fixed effects. Columns (2)-(7) report the matched DiD results where matching criteria changes in the order of distance threshold (200, 100 and 50 mi.) and nearest neighbors (10, 5, and 1). For the coal type matching criterion, predominant coal burned in 1997 is used.[20] Throughout the paper, I repeatedly report the OLS specification, and the matched DiD with the 10 nearest neighbor criterion as the default specifications.[21]

Table 1.3: DiD Estimates of Log(Price): Replication

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1(Divest)	-0.131***	-0.125***	-0.189***	-0.148*	-0.125***	-0.130***	-0.136**
	(0.041)	(0.043)	(0.058)	(0.077)	(0.044)	(0.046)	(0.063)
Coal Type Year	97	97	97	97	97	97	97
m Nearest Neighbors					10	5	1
Proximity Threshold (mi.)		200	100	50			
Year-Month FE		Yes	Yes	Yes	Yes	Yes	Yes
Div-Yr FE	Yes						
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.802	0.715	0.706	0.662	0.717	0.720	0.733
Total Plants	397	230	146	69	205	176	130
Divested Plants	88	87	74	39	87	87	87
Control Plants	309	143	72	30	118	89	43
Observations	86049	46848	28286	12594	37275	32749	23132

Notes: Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Standard errors are clustered at the plant level.

[19] Due to minor changes in the data cleaning process (treatment of duplicate observations, classification of electric plants vs. industrial plants, etc.), the replicated estimates differ slightly from the original estimates in Cicala. However, the magnitude of difference is minimal; the point estimates start to differ at most by 0.003.

[20] Cicala's sample includes 5 additional non-divested plants that are excluded in this paper. These plants have always operated as independent power producers and have never been subject to rate regulation. Historically, independent power producers were allowed to enter and provide electricity to regulated utilities since the Public Utility Regulatory Policies Act (PURPA) of 1978. But, the number was limited before electricity market deregulation.

[21] The 10 nearest neighbor criterion is also a preferred specification in Cicala.

1.4.2 Revised DiD Estimation

Table 1.4 shows that once the ComEd plants are excluded from the estimation, the DiD coefficient falls to a range of 3% to 6% instead of 12% and they become statistically insignificant. First, Columns (1) and (4) of Table 1.4 repeat the estimation results reported in Columns (1) and (5) of Table 1.3. Then, Columns (2) and (5) present estimation results after excluding the ComEd plants but keeping 1997 as the reference year for a plant's coal type. In Columns (3) and (6), I further refine the coal type definition to be all the pre-treatment years between 1990 and 1997. When the seven ComEd plants are excluded, the regression coefficients reveal that the effect of divestiture is between about 3% and 6%. The effect is marginally significant only with the 10 nearest neighbor matching without correcting for the coal type definition (Column 4). For the rest of the paper, the sample excludes the ComEd plants.

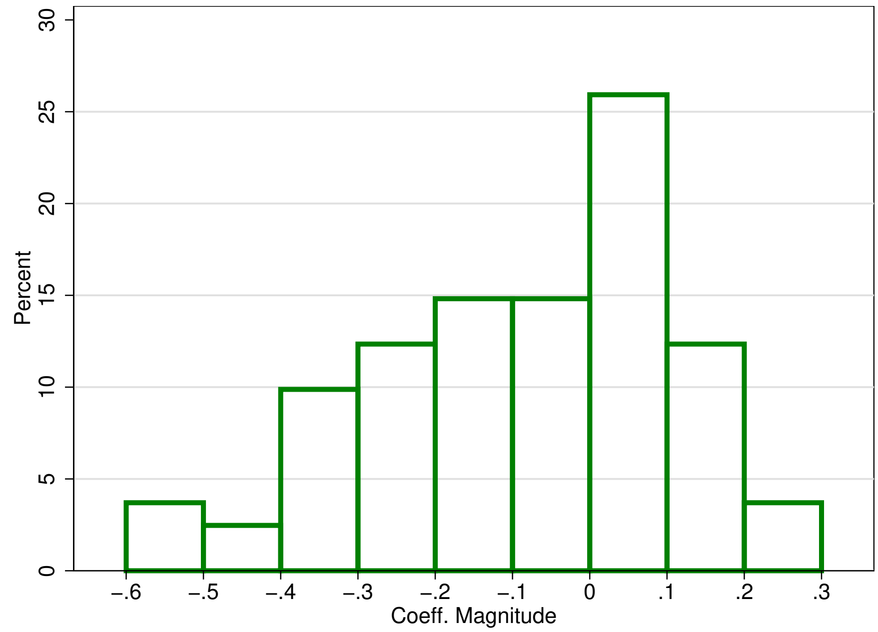
Table 1.4: DiD Estimates of Log(Price): Adjusting for the ComEd Plants

	(1) Full	(2) Adj.1	(3) Adj.2	(4) Full	(5) Adj.1	(6) Adj.2
1(Divest)	-0.131***	-0.037	-0.036	-0.125***	-0.067*	-0.060
	(0.041)	(0.027)	(0.028)	(0.044)	(0.037)	(0.038)
Coal Type Year	97	97	90-97	97	97	90-97
m Nearest Neighbors				10	10	10
Proximity Threshold (mi.)						
Year-Month FE				Yes	Yes	Yes
Div-Yr FE	Yes	Yes	Yes			
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.802	0.818	0.820	0.717	0.773	0.777
Total Plants	397	390	385	205	181	159
Divested Plants	88	81	81	87	80	81
Control Plants	309	309	304	118	101	78
Observations	86049	84642	84404	37275	33094	32431

Notes: "Full" indicates the full sample of divested plants including the ComEd plants. "Adj.1" indicates the adjusted sample excluding the ComEd plants without correcting for the coal type definition. "Adj.2" indicates the adjusted sample that excludes the ComEd plants and uses the coal type definition based on the years between 1990 and 1997. Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Standard errors are clustered at the plant level.

Although the revised effect of deregulation is statistically insignificant, it is possible to examine the full distribution of treatment effect and assess the comprehensive performance. This can be implemented by estimating a separate DiD regression (Equation 1.3.1) for each individual divested plant and its respective matches instead of pooling all the plants. Figure 1.4.1 illustrates that there is considerable heterogeneity in the treatment effect. While the mean is at about -0.06, the 25th and 75th percentiles are -0.24 and 0.07, respectively. Heterogeneity in the treatment effect is revisited in Section 1.6.

Figure 1.4.1: Distribution of Treatment Effect



Notes: The 10 nearest neighbor matching is used to estimate a DiD coefficient for individual divested plants. Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j.

1.5 Confounding Factor: the Title IV of the Clean Air Act Amendments

When Title IV of the Clean Air Act Amendments of 1990 (known as the Acid Rain Program) introduced a cap-and-trade market for SO_2, power plants had three options of compliance: buy emissions allowances, install abatement equipment called scrubbers, and/or switch to low-sulfur sub-bituminous coal. In theory, plants with high abatement costs could buy emission allowances. Yet, rate regulation incentivized plants to adopt scrubbers over other compliance methods. Averch and Johnson (1962) predicted that regulated plants prefer capital-intensive abatement equipment as their profit rates depend on capital expenditure. In a paper that studies coal-fired power plants' compliance decisions for a regulation on another chemical, nitrogen oxides, Fowlie (2010) confirms the Averch-Johnson effect empirically and further finds that divested plants are more likely to buy emission allowances than abatement equipment. She reasons that flexible methods such as allowance purchases provide divested plants an option value of later adjusting to the cheapest method while installing scrubbers would be an irreversible commitment. In the case of the SO_2 regulation, the same logic carries over to switching to cleaner sub-bituminous coal as divested plants have a somewhat flexible control over their future coal usage. Cicala (2015) verifies that rate-regulated (non-divested) plants disproportionately adopt scrubbers while more divested plants choose to switch to low-sulfur sub-bituminous coal.

Unfortunately, a diverging response by the divested plants to the environmental regulation confounds the treatment effect of divestiture. When cleaner sub-bituminous coal is also cheaper, it is difficult to identify why divested plants switch: whether it is for cost savings in a deregulated market or for the environmental compliance. On one hand, if divested plants can switch for cost savings, coal-switching would be a mechanism through which deregulation brings the cost reductions. On the other hand, disproportionate coal-switching can be the divested plants' compliance to the environmental regulation. Moreover, for most of the divested plants, the fuel cost data is missing between 1999 and 2002 when the ARP is implemented in 2000.[22] This implies that the econometrician cannot zoom into a time period in which divestitures have taken place but the environmental regulation has not. When the actual motives behind coal-switching are unidentified, a

[22]The market began in 1996 for dirtier plants. From 2000 onward, they received reduced annual allowances. Other remaining coal-fired plants also had to comply in 2000 when the second phase started. As a result, almost all of the coal power plants in the U.S. had to take action to comply with the SO_2 emissions policy in 2000.

failure to include coal-switching as a control variable leads to an omitted variable bias. Once coal-switching is accounted for, the treatment effect represents cost reductions from burning the same coal type. I interpret this as the lower bound of the treatment effect.

Although it is impossible to tease apart the exact contribution of the ARP and that of divestiture on coal-switching, one could take a glimpse of how divested and non-divested plants would have behaved absent the environmental regulation. By separately looking at plants with and without scrubbers prior to the ARP, I compare how different degrees of environmental compliance affect coal-switching behavior between the divested and the non-divested plants. The idea is that when scrubbers typically cleanse more than 90% of SO_2 emissions (EPA, 2002), one can attribute any additional coal-switching from the divested plants (relative to the non-divested plants) to switching for cost saving purposes instead of environmental compliance. However, I find that divested plants with scrubbers do not switch more than non-divested plants with scrubbers. That is, when divested plants do not need to further reduce SO_2emissions, they do not carry out additional coal-switching. This suggests that divestiture alone would have led to less fuel-switching. Yet, this does not grant that the environmental regulation alone explains all the fuel-switching. More plants would have remained rate-regulated and adopted scrubbers absent the market deregulation. Then, the best one can interpret coal-switching post 2000 is that it was a comprehensive response to both the market deregulation and the environmental regulation.

1.5.1 Differential Trends in Switching to Sub-bituminous Coal

First, I document that the divested plants disproportionately switch away from bituminous to sub-bituminous coal upon divestiture.[23] To evaluate the differential trend, I interact the treatment dummy for divested plants with dummies indicating the year relative to the divestiture year. Formally, I estimate:

$$\begin{aligned}\mathbf{1}(\text{sub-bit coal})_{it} = &\sum_{t'=-8, t'\neq 0}^{8} \beta_{t'} \mathbf{1}(divest)_i \mathbf{1}(t = t' \text{ years since divestiture})_t \\ &+ \gamma_i + \delta_t + \epsilon_{it}\end{aligned}$$

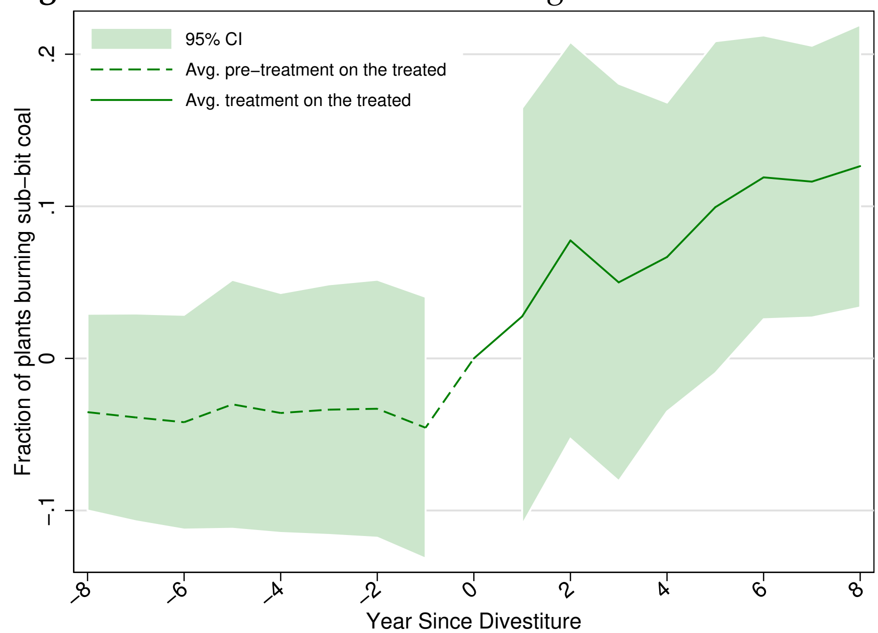
where $\mathbf{1}(divest)_i$ indicates a dummy equal to 1 if the plant i is ever divested, and $\mathbf{1}(t = t' \text{ years since divestiture})_t$ indicates a dummy equal to 1 if time t is t' years since divestiture. The omitted year is the year prior to divestiture. Note that the Acid Rain Program is implemented in 2000 and no divested plant reports the post-treatment cost data before 2000 due to the EIA's data collection rule. This means that the effect of divestiture on fuel-switching is identified only with the presence of the environmental regulation.

Figure 1.5.1 plots the estimated coefficients from the above estimating equation. In the regression, I drop the plants that use sub-bituminous coal in the pre-treatment period because they do not switch their coal type. The figure highlights a stark difference in the switching rate between the divested and the non-divested plants. Gradual instead of instantaneous switching by the divested plants suggests that certain plants have residual long-term contracts. Yet, the short-term effect should be interpreted with a caveat that it (i.e., less than 3 years since divestiture) is identified by a handful of plants that fully report the fuel cost data. By the end

[23] See Cicala (2015) for evidence that rate-regulated plants disproportionately choose to install scrubbers.

of the data in 2009, the number of divested "sub-bituminous" plants has increased about 3.1 times from 1997 while the same number for the non-divested plants has increased about 1.9 times from 1997.

Figure 1.5.1: Fraction of Plants Burning Sub-Bituminous Coal



Notes: Plants burning sub-bituminous coal in pre-treatment period are dropped. they The 10 nearest neighbor matching is used. Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Confidence intervals are based on standard errors clustered at the plant level.

Instead of looking at the time trend, one can also estiamte the average effect of divestiture and the Acid Rain Program on fuel-switching. Table DiD Estimates of $\mathbf{1}$(Sub-bit) reports the cofficient from regressing $\mathbf{1}(\text{sub-bit coal})_{it}$ on the post-treatment dummy. Divestiture together with the ARP implementation cause the probability of coal-switching to increase by about 13 percentage point (Column 3). The baseline probability or the fraction of sub-bituminous plants in the pre-treatment period for divested plants is about 9%.

Table 1.5: DiD Estimates of **1**(Sub-bit)

	(1)	(2)	(3)
1(Divest)	0.200***	0.130***	0.127***
	(0.045)	(0.043)	(0.044)
Coal Type Year		90-97	90-97
m Nearest Neighbors			10
Distance Threshold (mi.)		200	
Plant FE	Yes	Yes	Yes
Year FE		Yes	Yes
Div-Year FE	Yes		
R^2	0.822	0.711	0.711
Total Plants	385	210	159
Divested Plants	81	81	81
Control Plants	304	129	78
Observations	84404	43479	32431

Notes: Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Standard errors clustered at the plant level.

1.5.2 Effect of coal-switching on Delivered Fuel Costs

Differential rates of coal-switching between the divested and the non-divested plants raise a concern that the environmental compliance motives can be the source of fuel cost reductions among divested plants.[24] Although it is hard to identify what influneces coal-switching, one can still estimate how much of cost reductions is attributed to coal-switching. By controlling for coal-switching, I decompose the overall cost reductions into the "pure" divestiture component and the coal-switching component. If coal-switching is a mechanism through which divested

[24] To be fair, Cicala also addresses this concern. In order to show that coal-switching does not account for a large portion of cost reductions, he allows the treatment effect to differ between plants that switch coal types post divestiture and the ones that always burn the same coal. He finds that "while switching yields a larger drop (in fuel costs), it accounts for a relatively small fraction of the overall treatment effect" and concludes that divestiture is effective regardless of the switching behavior.

plants save input costs, then the overall effect of divestiture will be given by a sum of the coefficients on the coal-switching dummy and the treatment dummy. In constrast, if the environmental regulation is the only driver behind coal-switching, the divestiture effect will be given solely by the treatment dummy. Therefore, I interpret the coefficient on the treatment dummy as a lower bound for the effect of divestiture on cost reductions. Formally, I estimate the following equation:

$$log(p_{it}) = \gamma_i + \delta_t + \alpha \mathbf{1}(\text{sub-bituminous})_{it} + \beta \mathbf{1}(divest)_{it} + \epsilon_{it}$$

where $\mathbf{1}(\text{sub-bituminous})_{it}$ denotes a dummy equal to 1 if the plant i's predominant source of coal is sub-bituminous in time t. Note that the dummy of a plant that always burns one type of coal is absorbed by the plant fixed effects. Then,α is identified by plants that switch their coal type.

Table 1.6 reports the estimation results. They show that if divested plants are compared to the non-divested plants with the same coal type, divestiture has a less-than-3% effect on cost reductions. In other words, a half of the earlier 6% effect is driven by the compositional changes of the divested plants: more of them switch to cleaner and cheaper sub-bituminous coal. The large, significant coefficient on the sub-bituminous dummy confirms that coal-switching is indeed the main driver behind the cost reductions.

Table 1.6: DiD Estimates of Log(Price): Accounting for Coal Type Switch

	(1)	(2)	(3)
1(Divest)	-0.007	-0.038	-0.033
	(0.025)	(0.033)	(0.035)
1(Sub-bit)	-0.144***	-0.198***	-0.210***
	(0.019)	(0.033)	(0.034)
Coal Type Year		90-97	90-97
m Nearest Neighbors			10
Distance Threshold (mi.)		200	
Plant FE	Yes	Yes	Yes
Year FE		Yes	Yes
Div-Year FE	Yes		
R^2	0.826	0.789	0.790
Total Plants	385	210	159
Divested Plants	81	81	81
Control Plants	304	129	78
Observations	84404	43479	32431

Notes: $\mathbf{1}(\text{sub-bit})_{it}$ indicates the predominant source of coal of a plant i in time t. Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Standard errors clustered at the plant level.

1.5.3 Contribution to Coal-Switching

One could approximate how divested and non-divested plants would have behaved absent the environmental regulation. Because of an earlier regulation, the 1970 Clean Air Act, certain coal-fired plants had already been emitting less SO_2 than what was required by the Acid Rain Program in 2000. By restricting attention to these plants, I can compare differential behavior in coal-switching between the divested and the non-divested plants without worrying about the switching motives associated with the environmental compliance.

Plants that already emit less than the ARP standard are the ones that have entered since 1971. The 1970 Clean Air Act Amendments mandated new generating units to meet the New Source Performance Standard, which "effectively required new coal-fired plants to install flue gas desulfurization equipment or a scrubber" (Ellerman, 2003).[25] As a consequence, these plants had minimal compliance requirement, if not at all, at the onset of the ARP in 2000 (Ellerman et al., 2000).

By separately looking at plants with and without scrubbers by 1990 (the year of the ARP legislation), I compare how different degrees of compliance requirement in emissions affect coal-switching behavior between the divested and the non-divested plants. The idea is that when scrubbers cleanse more than 90% of SO_2 emissions, one can attribute any additional coal-switching from the divested plants (relative to the non-divested plants) to cost savings instead of compliance responses. Formally, I estimate the following equation:

$$\begin{aligned}\mathbf{1}(\text{sub-bit coal})_{it} = &+ \beta_0 \mathbf{1}(divest)_{it} \mathbf{1}(scrub\, by\, 1990)_i \\ &+ \beta_1 \mathbf{1}(divest)_{it} \mathbf{1}(No\, scrub\, by\, 1990)_i \\ &+ \gamma_i + \delta_{gt} + \epsilon_{it}\end{aligned}$$

where δ_{gt} indicates a group-specific time fixed effects and $\mathbf{1}(scrub\, by\, 1990)_i$ and $\mathbf{1}(No\, scrub\, by\, 1990)_i$ respectively indicate a dummy equal to 1 if a plant has installed a scrubber before 1990 and if a plant has not installed a scrubber by 1990. Table DiD Estimates of 1(Sub-bit): By Scrubber Status presents the estimation results. I find that divested plants with scrubbers installed pre-1990 switch just as much as the similar non-divested plants. That is, when divested plants do not need to lower their SO_2 emissions, they do not carry out additional coal-switching. When divested plants do need to lower their emissions (i.e., plants without scrubbers by

[25]The history of the Clean Air Act dates back to 1963. Since then, amendments were enacted in 1970, 1977 and 1990. The New Source Performance Standard was first introduced in 1970 and a stricter provision was mandated in 1977. Then, the plants had to either install scrubbers or switch to low-sulfur coal. These regulations predate the Acid Rain Progrm which was added in 1990.

1990), they are 16 percentage point more likely to switch to sub-bituminous coal than the comparable non-divested plants. No switching by the scrubber-installed divested plants suggests that divestiture alone, absent the environmental regulation, would have led to less fuel-switching. Yet, one should interpret the results with a grain of salt: the treatment effect for the divested plants with scrubbers does not differ significantly from that for the divested plants without scrubbers (see the p-values from the Wald test on the difference between the DiD coefficients reported in the table as $H_0 : \beta_0 = \beta_1$).

Table 1.7: DiD Estimates of 1(Sub-bit): By Scrubber Status

	(1)	(2)	(3)
1(Divest)*1(Scrubber by 1990)	0.164	0.014	0.008
	(0.105)	(0.097)	(0.100)
1(Divest)*1(No scrubber by 1990)	0.213***	0.161***	0.160***
	(0.052)	(0.046)	(0.048)
p-value($H_0 : \beta_0 = \beta_1$)	0.673	0.172	0.173
Coal Type Year		90-97	90-97
m Nearest Neighbors			10
Distance Threshold (mi.)		200	
Plant FE	Yes	Yes	Yes
Div-Yr-Cohort FE	Yes		
Time-Cohort FE		Yes	Yes
R^2	0.824	0.716	0.715
Total Plants	385	210	159
Divested Plants	81	81	81
Control Plants	304	129	78
Observations	84404	45100	33859

Notes: "SubShare" is defined to be a sub-bituminous share of the total coal delivery to the firm i in time t. The matched sample excludes non-divested plants with the distance more than 200 miles from the closest divested plant. For each divested plant j, non-divested plants are matched if they use the same coal rank as j. Coal rank of a plant is based on all the pre-trement years from 1990 to 1997. Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Confidence intervals based on standard errors clustered at the plant level.

In particular, I do not assert that the environmental regulation explains all the fuel-switching. In theory, more plants would have installed scrubbers abset the market deregulation. The best one can interpret coal-switching post 2000 is that it was a combined response to both the market deregulation and the environmental regulation.

1.6 Heterogeneous effect

Whether or not one accounts for coal-switching, deregulation has a limited impact on a plant's procurement costs. However, this does not imply that deregulation has failed altogether. When power plants purchase coal via bilateral long-term contracts, an amount of concessions a plant can extract from suppliers depends on the plant's incentive and/or ability to negotiate.

First, I observe that plants stuck in unfavorably high cost contracts could seize divestiture as an opportunity to bargain better terms. Though contracts are typically difficult to break, they are not impossible to re-negotiate. In fact, coal contracts have become more sophisticated in enlisting numerous reopener clauses since the 1970's. Major changes in environmental policies and high inflation rates deemed negotiated prices reasonable one day but unacceptable the next day. More recent contracts typically include provisions that could trigger a re-negotiation a) "when government regulations that were not anticipated by either part when the contract was executed are imposed" and b) when government regulations on emissions contrain the ability of the buyer to make use of the quantities of coal that have been contracted for" Joskow (1988). Although detailed data on contract beginning and expiration are limited, analyzing the Coal Transportation Rate Database of the EIA reveals that contracts, on average, were signed about 8.3 years previous to 1997 and had about 4.6 years remaning until expiration as of 1997. It is unlikely that either mines or plants had anticipated market deregulation at the time of signing such long-term contracts. In theory, an unanticipated change in regulatory regime such as divestiture would provide plants (or mines) an opportunity to bargain new contract terms. I find that divested plants with highly unfavorable (high-cost) contracts relative to the neighbors do achieve substantial cost reductions via re-negotiations.

Second, literature on countervailing power predicts that bigger buyers obtain larger discounts from suppliers. Among numerous competing stories, a simple theory provides an exposition that per-unit production costs are lower in serving a larger buyer if a supplier's production function has increasing returns to scale.[26] In the case of coal, Boyd (1987) notes that large drilling and mining equipment are associated with large output volumes of coal mines, and estimates that coal production exhibits increasing returns to scale. Willamson explains the large buyer discount via a concept of "dedicated assests" (1983). Joskow summarizes the concept as: "the larger the annual quantity of coal that is contracted for, the more difficult it is for the seller to quickly dispose of unanticipated supplies at a compensatory price" (1987*a*). When losing a larger buyer is costly, coal suppliers will submit to lower rates. I proxy a plant's size with its production capacity and show that plants with a bigger capacity significantly lower their fuel costs upon divestiture.

Before proceeding further, I point out a caveat that the analysis is performed on the plant level instead on the operator level. ComEd's re-negotiation in 1993 suggests that an umbrella operating company may be a representative negotiator

[26]See papers listed in related literature and a survey by Snyder (2005) for other competing theories.

for all the fuel delivered to its plants. Technically, one can repeat the analysis on the operator level. However, a couple of reasons grant the plant level analysis more appropriate. First, divestiture by definition entails a change in operator, at least legally. Then, an old operator makes an "exit" and a new operator makes an "entry" from the econometrician's point of view. Econometric analysis would drop observations from the "exiting" or "entering" operators because either pre- or post-treatment data is "missing."[27] Second, an average and a median number of plants per operator is respectively 2.02 and 1 suggesting that any given operator is likely to be responsible for managing, at most, a few plants. Lastly, contracts reported in Coal Transportation Rate Database deliver to 1.7 plants on average (median 1 plant). Because plant operators make contract decisions based on a few plants, the plant level analysis would not deviate too much from the operator level analysis. Nonetheless, I state my assumption that I take contracts as plant-specific in interpreting the estimation results.

1.6.1 Disadvantaged Coal Contracts

This section shows that divested plants with unfavorable contracts reduce their costs while the ones with no disadvantage maintain the status quo. First, I construct a measure of contract disadvantage. This variable is then interacted with the DiD term to estimate a heterogeneous treatment effect of how different degrees of contract disadvantage affect cost reductions. I define the disadvantage term as the percentage difference between the own contract price and the average contract

[27]This problem can be potentially remedied by figuring out an operator's ultimate parent company. Power plant database from Platts keeps a record of a plant's ultimate parent company. Yet, in many cases, multiple plants previously owned by a single utility are not necessarily sold off to a single operator. Then, the problem of assigning a proper operator is further complicated. Two plants sold off to different operators would have a single previous operator.

price at the neighboring plants. Formally, for a divested plant k,

$$\text{Contract disadvantage} \equiv \frac{p^c_{k0} - \bar{p^c_{j0}}}{\bar{p^c_{j0}}}, \text{ where}$$

$$p^c_{k0} = \sum_{t=Jan.1990}^{Dec.1997} \frac{(\text{MMBtu delivered under contract } * \text{Cost of contract in dollars})_{kt}}{(\text{Total MMBtu delivered under contract})_k}$$

$$\bar{p^c_{j0}} = \sum_{j\in K} \frac{1}{N_K} \sum_{t=Jan.1990}^{Dec.1997} \frac{(\text{MMBtu delivered under contract } * \text{Cost of contract in dollars})_{jt}}{(\text{Total MMBtu delivered under contract})_j}$$

and K indicates a set of neighboring plants j near plant k.

The neighboring plants of plant k include either divested or non-divested plants that use the same rank coal as k. Like in the matching procedure, there is arbitrariness in the choice of which plants constitute the neighbors. However, the estimation results are robust to different choices of neighbors. Lastly, it is important to note that the contract price of coal is different from the overall price of coal, the outcome variable in Equation 1.3.1. The overall price of coal delivered to a firm is the quantity-weighted (i.e., MMBtu-weighted) average of both the contract and the spot market prices. The disadvantage term is based only on the contract prices.

With the interaction term, the new DiD estimating equation becomes:[28]

$$log(p_{it}) = \gamma_i + \delta_t + \beta_0 \mathbf{1}(divest)_{it} + \beta_1 \mathbf{1}(divest)_{it}(Disadv.)_i + \epsilon_{it},$$

where $Disadv.$ indicates a contract disadvantage measure, defined in Equation 1.3.1. However, the above equation produces biased estimates. Because the pre-treatment contract prices appear both on the left-hand and on the right-hand side, the plant fixed effects introduce a simultaneity bias (Nickell, 1981). The size of

[28]To allow for different time trends between the disadvantaged and the non-disadvantaged plants, one can interact the time fixed effects with the contract disadvantage term. Such specification produces similar results.

the bias will be proportional to the inverse of the time dimension, T, of the data. Because the data is in monthly units for multiple years, the bias should be small. Regardless, an instrumental variable approach is adopted to restore the unbiased estimates.

Table 1.8 reports the heterogeneous treatment effect by a plant's pre-period disadvantage. Columns (1)-(2) report OLS DiD estimates. In constructing a spread between the own contract price and the neighbor contract prices, the 10 nearest neighbors are considered. Columns (3)-(6) report the results with matched DiD estimations. In Columns (1), (3) and (5), significant negative coefficients on the interacted DiD term imply that the divested plants that pay a premium above their neighbors reduce costs upon divestiture. Yet, a closer look at the data reveals that most of cost reductions by the divested plants come from the plants with the most disadvantaged contracts in the top 25% (Columns (2), (4) and (6)). Those plants reduce costs by about 18% ($\beta_0 + \beta_1$ from Column(6)). It is intuitive that that plants without substantial saving prospects would not leverage deregulation. When they would not earn substantially better rates than the prevaling market price, the benefits of re-negotiation do not necessarilly outweigh the costs.

Table 1.8: DiD Estimates of Log(Price): By Contract Disadvantage

	(1)	(2)	(3)	(4)	(5)	(6)
1(Divest)	0.018	0.061**	-0.018	0.019	-0.016	0.026
	(0.022)	(0.025)	(0.033)	(0.033)	(0.035)	(0.035)
1(Divest)*Disadv.	-0.442***		-0.489***		-0.499***	
	(0.144)		(0.113)		(0.117)	
1(Divest)*1(Disadv.: 4th Q.)		-0.190***		-0.206***		-0.208***
		(0.041)		(0.047)		(0.045)
1(Sub-bit)	-0.147***	-0.152***	-0.169***	-0.174***	-0.180***	-0.191***
	(0.020)	(0.020)	(0.032)	(0.032)	(0.033)	(0.033)
Coal Type Year			90-97	90-97	90-97	90-97
m Nearest Neighbors					10	10
Distance Threshold (mi.)			200	200		
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes
Div-Yr FE	Yes	Yes				
Year-Month FE			Yes	Yes	Yes	Yes
R^2	0.831	0.831	0.797	0.798	0.800	0.801
Total Plants	368	368	207	207	157	157
Divested Plants	81	81	81	81	81	81
Control Plants	287	287	126	126	76	76
Observations	81267	81267	44634	44634	33594	33594

Notes: "*Disadv.*" of a plant i is defined to be a percentage difference between the average coal prices of i and those of the neighbors. "$1(Disadv.: 4th\,Q.)$" is a dummy equal to 1 if a disadvantage measure of plant i falls into the 4th quantile. Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Standard errors are clustered at the plant level.

Though the magnitude of bias decreases as the sample size, especially the time dimension, increases, one may still worry that the bias could overturn the estimation results as the direction of bias is unclear.[29] However, proper instrumental variables can successfully address the endogeneity issue. Ideal instruments need to explain the degree of contract unfavorableness (i.e., spread between the own and the neighbors' contract prices) while uncorrelated with the unobserved errors. I use environmental quality of coal as the instruments. For a divested plant k, the first IV is the percentage difference between the average of k's own ash contents of

[29] In a dynamic panel model, the estimated coefficient is inflated (deflated) if the true coefficient is positive (negative) (Nickell, 1981).

coal delivered under contract and that of k's neighboring plants. The second IV is an indicator variable of whether or not a plant belongs to the Phase I of the 1990 Clean Air Act Amendments. Formally, the IV's are: $\frac{x_{i0}-\bar{x_{j0}}}{\bar{x_{j0}}}$ and $\mathbf{1}(CAA\ Phase\ I)$ where x indicates the ash content of coal delivered under contract, the bar indicates the average across the neighboring plants and $\mathbf{1}(CAA\ Phase\ I)$ indicates a dummy equal to 1 if a plant is subjective to the Phase 1 of the Clean Air Act. In general, ash content of coal is negatively correlated with the coal prices because higher ash content increases maintenance costs. Then, it is plausible that unfavorable contracts offer not only more expensive rates but also worse quality coal. Percentage difference in ash content is a proxy measure of contract unfavorability.

For the exogeneity condition, the nature of coal contracts deems it likely that unobserved fluctuations (errors) in the coal prices are uncorrelated with the environmental measures of coal. Terms of a contract such as coal prices and specifications of environmental contents are typically locked in upon signing the contract (Joskow, 1985, 1987*a*). When the relation between the coal prices and the environmental measures is formulaic, the changes in the environmental measures should all be reflected in the observed prices but not in the unobserved components of the prices. In other words, sulfur and ash contents of coal affect the contract prices in a pre-determined manner as specified under the terms of contract but not in any other unobserved way.

However, there still is an exogeneity concern that shipping rates, an unobserved portion of the delivered prices, are set based on environmental quality of coal being delivered. Busse and Keohane (2007) finds that rail companies are able to discriminate power plants based on whether or not the plant is subjective to SO_2 emissions compliance. If ash content of coal is perfectly correlated with the sulfur content, then the ash content would be correlated with the unobserved shipping costs. However, there is no obvious geological relation between the sulfur and the ash contents of coal.[30] Moreover, the rail companies, in practice, identified dirtier plants based on whether or not a plant was subjective to the earlier phase of the Clean Air Act. It is unclear whether the rail companies had the authority to measure and discriminate specifically on the actual sulfur contents of coal. To address for the potential differences in unobserved shipping costs, I explicitly control for whether or not a plant is subjective to the earlier phase of the Clean Air Act.

Table 1.9 reports the IV DiD estimation results. Columns (1) repeats the DID estimation results without using any IV's. Column (3) presents a specification with the afore-mentioned ash and Clean Air Act IV's and Column (2) is the first stage regression. The first stage regression suggests that the dirtier Phase I plants actually pay lower prices than the Phase II plants though the difference is not statistically significant. Yet, including the Clean Air Act dummy creates a potential weak IV problem. A specification only with the ash IV is reported in Columns (4) and (5). Once the Clean Air Act IV is removed, the first stage F-stat is about 14. The estimated coefficients are more negative than the specification without the IV.

[30] Ash typically consists of inorganic matter from the earth's crust (USGS, 2009). In contrast, "much of the sulfur derives from the sulfur content of the plant material making up the original peat" (Calkins, 1994).

Table 1.9: IV DiD Estimates of Log(Price): By Contract Disadvantage

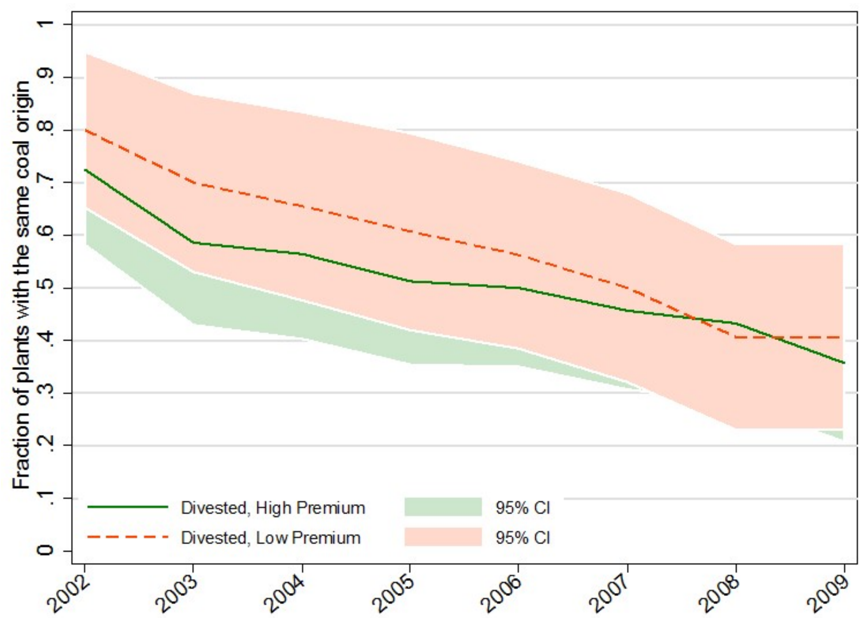
	(1) No IV	(2) 1st	(3) IV	(4) 1st	(5) IV
Ash Disadv.		0.271***		0.291***	
		(0.084)		(0.078)	
1(Phase 1 ARP)		-0.131			
		(0.151)			
1(Divest)	0.026		0.080**		0.090**
	(0.035)		(0.035)		(0.041)
1(Divest)*1(Disadv.: 4th Q.)	-0.208***		-0.291***		-0.331***
	(0.045)		(0.076)		(0.073)
1(Sub-bit)	-0.191***		-0.053***		-0.053***
	(0.033)		(0.018)		(0.018)
First stage F-stat		11.975		14.072	
Coal Type Year	90-97		90-97		90-97
m Nearest Neighbors	10		10		10
First Difference			Yes		Yes
Plant FE	Yes				
Year-Month FE	Yes		Yes		Yes
R^2	0.801		0.021		0.020
Total Plants	157		157		157
Divested Plants	81		81		81
Control Plants	76		76		76
Observations	33594		32650		32650

Notes: "*Disadv.*" of a plant i is defined to be a percentage difference between the average coal prices of i and those of the neighbors. "$1(Disadv.: 4th\,Q.)$" is a dummy equal to 1 if a disadvantage measure of plant i falls into the 4th quantile. Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Standard errors are clustered at the plant level.

1.6.1.1 Evidence of Contract Re-negotiation

This section provides some evidence that cost reductions arise from contract re-negotiations as well as finding new contracts. Because the EIA database does not collect information on supplier names prior to 2002, it is impossible to directly check whether plants change their coal suppliers before and after divestiture. However, origin county of coal deliveries are observed, and any departure from the original delivery source can proxy for whether contracts are terminated or re-negotiated upon divestiture. Figure 1.6.1 plots a fraction of plants with the same coal origin as before divestiture. It reveals that about 50% of the divested plants with the high disadvantage retained their deliveries from the same county as before the divestiture. However, because a number of coal suppliers exist in a single county, this analysis ignores changes in suppliers within the same county and is confined to changes in suppliers across counties.

Figure 1.6.1: Trends in a Proxy for Plant Re-negotiation



Notes: "High premium" indicates plants with the disadvantage term above the 75th percentile. "Low premium" indicates plants with the disadvantage term below the 75th percentile. Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Confidence intervals are based on clustered standard errors at the plant level.

Conditional on the same delivery source, divested plants were indeed able to reduce costs. In order to separate the cost reductions due to re-negotiation from those due to switching in coal counties, I estimate the following equation:

$$
\begin{aligned}
log(p_{it}) = & + \beta_0 \mathbf{1}(divest)_{it}\mathbf{1}(Same\,County)_{it} \\
& + \beta_1 \mathbf{1}(divest)_{it}\mathbf{1}(Same\,County)_{it}(Disadv.)_i \\
& + \beta_2 \mathbf{1}(divest)_{it}\mathbf{1}(Diff.County)_{it}, \\
& + \beta_3 \mathbf{1}(divest)_{it}\mathbf{1}(Diff.County)_{it}(Disadv.)_i \\
& + \gamma_i + \delta_{gt} + \epsilon_{it}
\end{aligned}
$$

where $\mathbf{1}(Same\,County)_{it}$ indicates a dummy equal to 1 if a plant continues to have coal deliveries from the same county as before the treatment and $\mathbf{1}(Diff.County)_{it}$ indicates a dummy equal to 1 if a plant switches its coal delivery county after the treatment. Large negative coefficients on the disadvantage term interacted with the same county indicator suggest that cost reductions are made by the divested plants without switching the delivery counties.

Table 1.10: DiD Estimates of Log(Price): By Contract Disadvantage and Renegotiation

	(1)	(2)	(3)	(4)	(5)	(6)
1(Divest)*1(Same)	0.005	0.088**	-0.065	0.023	-0.061	0.026
	(0.043)	(0.034)	(0.054)	(0.046)	(0.056)	(0.048)
1(Divest)*1(Diff.)	-0.011	0.019	-0.033	-0.005	-0.028	-0.003
	(0.027)	(0.025)	(0.034)	(0.033)	(0.036)	(0.035)
1(Divest)*1(Disadv.: 4th Q.)*1(Same)		-0.322***		-0.365***		-0.359***
		(0.094)		(0.099)		(0.100)
1(Divest)*1(Disadv.: 4th Q.)*1(Diff.)		-0.371**		-0.435***		-0.441***
		(0.174)		(0.103)		(0.108)
1(Sub-bit)	-0.143***	-0.147***	-0.197***	-0.171***	-0.206***	-0.183***
	(0.019)	(0.020)	(0.032)	(0.031)	(0.032)	(0.033)
Coal Type Year			90-97	90-97	90-97	90-97
m Nearest Neighbors					10	10
Distance Threshold (mi.)			200	200		
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes
Div-Yr FE	Yes	Yes				
Year-Month FE			Yes	Yes	Yes	Yes
R^2	0.826	0.832	0.792	0.799	0.795	0.802
Total Plants	385	368	210	207	159	157
Divested Plants	81	81	81	81	81	81
Control Plants	304	287	129	126	78	76
Observations	84404	81267	45100	44634	33859	33594

Notes: "$1(Disadv.: 4th\,Q.)$" is a dummy equal to 1 if the disadvantage measure of plant i falls into the 4th quantile. "$1(Same)$" is a dummy equal to 1 if a plant i continues have its delivery from the same county as before the treatment. "$1(Diff.)$" is a dummy equal to 1 if a plant i discontinues its delivery from the pre-treatment delivery county. Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Standard errors are clustered at the plant level.

1.6.2 Plant Size and Bargaining Leverage

In order to estimate the heterogeneous treatment effect of a plant's size, I interact the DiD term with a plant's production capacity. Though a plant's capacity can be time-varying, divestiture itself does not lead to capacity upgrades or downgrades. In other words, divestiture does not affect fuel procurement costs indirectly through changes in capacity. Formally, I estimate the following equation:

$$log(p_{it}) = \alpha_0 Capacity_{it} + \beta_0 \mathbf{1}(divest)_{it} + \beta_1 \mathbf{1}(divest)_{it} Capacity_{it} + \gamma_i + \delta_t + \epsilon_{it},$$

where $Capacity_{it}$ indicates a plant's nameplate capacity in 100 megawatts. Table 1.11 presents the estimation results. Columns (1)-(2) report OLS DiD estimates. Columns (3)-(6) report the estimation results from a matched DiD with the 200 mi. distance caliper and the 10 nearest neighbors. Columns (1), (3) and (5) include a linear capacity term whereas Columns (2), (4) and (6) include a dummy equal to 1 if a plant's capacity is greater than the median. The treatment effect for divested plants with capacity one standard deviation (730 megawatts) above the average is about 12 ($\alpha_0 7.3 + \beta_0 + \beta_1 7.3$ from column(5)) percentage point more than that for the divested plants with the average capacity.

Table 1.11: DiD Estimates of Log(Price): By Plant Size

	(1)	(2)	(3)	(4)	(5)	(6)
1(Divest)	0.087**	0.059*	0.076**	0.028	0.080**	0.032
	(0.034)	(0.034)	(0.038)	(0.037)	(0.040)	(0.039)
Nameplate	-0.004		-0.012		-0.016*	
	(0.004)		(0.008)		(0.008)	
1(Divest)*Nameplate	-0.009***		-0.012***		-0.012***	
	(0.002)		(0.002)		(0.002)	
1(Nameplate > Median)		0.014		-0.070*		-0.094**
		(0.031)		(0.038)		(0.037)
1(Divest)*1(Nameplate > Median)		-0.110***		-0.125***		-0.124***
		(0.035)		(0.037)		(0.037)
1(Sub-bit)	-0.147***	-0.146***	-0.211***	-0.206***	-0.223***	-0.219***
	(0.019)	(0.019)	(0.032)	(0.031)	(0.032)	(0.032)
Coal Type Year			90-97	90-97	90-97	90-97
m Nearest Neighbors					10	10
Distance Threshold (mi.)			200	200		
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes
Div-Yr FE	Yes	Yes				
Yr-Mnth FE			Yes	Yes	Yes	Yes
R^2	0.828	0.827	0.797	0.793	0.798	0.795
Total Plants	385	385	210	210	159	159
Divested Plants	81	81	81	81	81	81
Control Plants	304	304	129	129	78	78
Observations	84404	84404	43479	43479	32431	32431

Notes: Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Standard errors are clustered at the plant level.

However, one might be concerned: a small (big) plant in fact belongs to a larger (smaller) operator and it may gain windfall (incur smaller) concessions in fuel prices thanks to the umbrella operator's negotiating efforts. If such bias exists, it would deflate the reported coefficient on the plant's capacity. To address this issue, I account for a size of an operator that the plant belongs to. Table 1.12 reports regression estimates after controlling for various measures of an operator's size. Column (1) and (4) present the baseline estimates identical to Columns (1) and (5) in Table 1.11. In Column (2) and (5), I expand the estimating equation to include the total number of plants that the umbrella operator owns. The coefficient estimate on the operator's total number of plants is -0.005, implying that increasing by 1 the number of plants operated by the same operator induces cost reductions by about 0.5 percentage points. When the maximum number of plants an operator owns is 11, a change from the minimum to the maximum for this variable would lower fuel costs by about 5.5 percentage points. Given that a standard deviation increase in a plant's capacity yields a 12 percentage point increase in the treatment effect, additional cost reductions from having another plant owned by the same operator are modest. Columns (3) and (6) report specification with an alternative measure of an operator's size, total capacity owned by the operator in 100 megawatts. Again, the coefficients on the total capacity measure are small. Statistically insignificant and economically small coefficients on the operator's size suggest that a plant's bargaining leverage follows mostly from its own size rather than the operator's size.

Table 1.12: DiD Estimates of Log(Price): By Plant and Owner Size

	(1)	(2)	(3)	(4)	(5)	(6)
1(Divest)	0.087**	0.089***	0.087**	0.080**	0.082**	0.081**
	(0.034)	(0.034)	(0.034)	(0.040)	(0.039)	(0.039)
Nameplate	-0.004	-0.004	-0.004	-0.016*	-0.020**	-0.019**
	(0.004)	(0.004)	(0.004)	(0.008)	(0.009)	(0.009)
1(Divest)*Nameplate	-0.009***	-0.009***	-0.009***	-0.012***	-0.012***	-0.012***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Number of plants owned		-0.005			-0.010	
		(0.006)			(0.007)	
Total capacity owned			-0.000			-0.001
			(0.000)			(0.001)
1(Sub-bit)	-0.147***	-0.146***	-0.147***	-0.223***	-0.219***	-0.217***
	(0.019)	(0.019)	(0.019)	(0.032)	(0.032)	(0.032)
Coal Type Year				90-97	90-97	90-97
m Nearest Neighbors				10	10	10
Distance Threshold (mi.)						
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes
Div-Yr FE	Yes	Yes	Yes			
Yr-Mnth FE				Yes	Yes	Yes
R^2	0.828	0.828	0.828	0.798	0.799	0.799
Total Plants	385	385	385	159	159	159
Divested Plants	81	81	81	81	81	81
Control Plants	304	304	304	78	78	78
Observations	84404	84375	84375	32431	32402	32402

Notes: Matched non-divested plants receive weight $\frac{1}{m_j}$ for each divested facility j where m is the number of matches to j. Standard errors are clustered at the plant level.

1.7 Conclusion

This paper revisits the impact of deregulation on coal procurement costs of the U.S. power plants. Though critical in providing the first evidence on how deregulation impacted the largest portion of operating expenses, the earlier finding by Cicala (2015) contains a set of plants that exhibit differential pre-trends. I also show that the environmental regulation has induced divested plants to disproportionately switch to cleaner and yet cheaper sub-bituminous coal. When the environmental regulation coincides with plant divestiture, it is rather difficult to identify how much of coal-switching and associated cost reductions are due to the environmental compliance versus the market deregulation. I estimate that divestiture leads to about 3%˜6% cost reductions, yet at statistically insignificant levels.

However, certain plants do achieve substantial cost savings upon divestiture. First, expensive contracts encourage plants to leverage divestiture to bargain for lower rates. Second, a potential for bigger purchases allows plants to negotiate cheaper rates. Future researchers and policy makers should note that deregulation is most successful when plants have incentives and abilities to negotiate their fuel contracts.

Chapter 2

Mergers and Supplier Networks in the U.S. Power Sector[31]

2.1 Introduction

In many business-to-business interactions where bilateral negotiations are at the heart of economic transactions, mergers have been regarded as a tool to strengthen bargaining leverage. This belief has its roots in the concept of countervailing power (Galbraith, 1952) that larger buyers can negotiate lower prices. Formally, Stole and Zwiebel (1996) and Chipty and Snyder (1999) provide a theoretical foundation for the idea. They show that in a simple bargaining between a monopoly supplier and non-competing buyers, a disagreement with a merged, hence larger, buyer lowers the supplier's bargaining surplus more than one with a smaller buyer, resulting in lower prices for the larger buyer.[32] Although generalization of the concept is

[31] Joint with Jose Miguel Abito, Jean-François Houde, Nathan Miller, and Matthew Weinberg

[32] The intuition for their result arises from the inverse Jensen's inequality. If a supplier's aggregate surplus from bargaining is concave in quantity (e.g., convex cost function), then taking away a larger quantity from the supplier results in a lower per-unit surplus than taking away a smaller quantity. The relationship between buyer size and discounts is also empirically documented. Sorensen (2003) finds that larger health insurers obtain better prices from hospitals. Ho

subtle if buyers compete with each other (Iozzi and Valletti, 2014), an empirical bargaining literature including Gowrisankaran, Nevo and Town (2015) and Ho and Lee (2017) has adopted the framework of countervailing power as a workhorse behind a merged firm's bargaining leverage in bilateral oligopoly settings.[33]

Countervailing power, however, is not the only mechanism through which a firm may gain its bargaining leverage. A growing literature on bargaining with endogenous network formations focuses on an extensive margin: a threat to exclude suppliers. For example, Ho and Lee (2016) allows firms to use a threat of replacing an existing supplier with an alternative and Liebman (2016) allows firms to commit to the maximum number of suppliers and exclude the rest. In either case, a threat of excluding suppliers gains bargaining leverage for the buyers. Yet, a failure to account for endogenous network formation does not grant that the empirical results are biased in the classic (so-called Nash-in-Nash) bargaining literature.[34] This is because in certain industries, firms have no room to adjust their relationships with the suppliers. For example, a government regulator constrains the network of insurers and hospitals to be nearly complete in California in 2004 (Ho and Lee, 2016).[35] Nonetheless, the general validity of a bargaining model

(2009) finds the opposite holds as well that hospitals in a larger hospital system extract higher reimbursement rates from health care providers. Ellison and Snyder (2010) finds that larger drugstores obtain better prices for off-patent and generic antibiotics from the pharmaceutical companies. Lakdawalla and Yin (2015) finds that larger health insurers negotiate lower drug prices for their enrollees from the pharmacies. Outside of the health care industry, Chipty (1995) finds that larger cable providers bargain for lower input prices and Normann, Ruffle and Snyder (2007) finds evidence for volume discounts in a lab experiment.

[33]For example, Ho and Lee (2017) shows that if consumer substitution is sufficiently limited across downstream firms (i.e., insurers) and dependence of rival firms of a merged downstream firm on the merged firm's supplier is also sufficiently limited, the logic of countervailing power holds that a disagreement with the merged downstream firm can still lower the supplier's bargaining surplus more than one with a smaller buyer.

[34]"Nash-in-Nash" bargaining refers to a Nash equilibrium in a Nash bargaining game (Collard-Wexler, Gowrisankaran and Lee, 2016).

[35]Ho and Lee (2016) finds that if a government intervention sets firm networks, then the predictions from a bargaining model with a threat of exclusion coincides those from a model without. The intuition is that as a firm contracts with almost all of the suppliers, it cannot find an alternative

without an endogenous network formation would hinge on rather rare opportunities like nearly full networks in the cable industry or government interventions in the health care industry.

While the exogeneity assumption on firm networks is fundamentally an empirical question, there is a lack of direct empirical evidence on whether and how firms form endogenous networks.[36] In this paper, we empirically document the formation of endogenous networks. Specifically, we provide the first evidence that downstream mergers induce the merging entities to consolidate their supplier base. Our empirical setting is the U.S. electricity/coal sector where coal-fired power plants procure a majority of their fuel input via bilateral contracts with coal suppliers. We begin our analysis by showing that power plants rely on a small number of suppliers despite their access to a larger pool of suppliers. We, then, show summary statistics on merger and acquisition (M&A) activities of power companies. In particular, we present evidence that M&A's are associated with supplier switching, a precursor of supplier consolidation. Then, we define measures of supplier consolidation at the plant pair level because supplier consolidation naturally involves comparing procurement decisions at multiple plants, two at the least. Lastly, we estimate the impact of M&A's on various measures of supplier consolidation.

Before we proceed, we address a few endogeneity problems with testing whether M&A's lead to supplier consolidation. First is a selection bias. In general, decisions to merge or acquire plants are choices made by firms and therefore not random.

supplier who is willing to replace an existing supplier at a satisfactory price.

[36] Atalay, Hortaçsu and Syverson (2014) and Carvalho and Voigtländer (2014) are few exceptions that study linkages between firms. However, they often define a linkage between an upstream and a downstream firm based on a fraction of the upstream firm's sales or revenue that the downstream firm accounts for. While a large customer of a supplier is more likely to be engaged in a strategic negotiation with the supplier, this does not grant that firms are in contractual relationships.

Yet, such decisions are often based on an entire group of plants as a whole (i.e., the total number of pairs between the two groups) rather than on a particular pair of plants between the merging firms. In particular, if a firm M&A involves combining large groups of plants, then any two plants of the merging firms are less likely to be instrumental to the firm-level decision. Empirically, we observe that power company M&A's involve combining about 6.4 plants from the acquiring firm with about 4.3 plants from the acquired firm. As a result, a M&A on average involves a total number of 27.5 (6.4*4.3) plant pairs.[37] So, we argue that any particular pair of plants of merging firms is exogenous to the firm level M&A decision. This argument is similar to Hastings (2004) and many of the retrospective merger analysis papers in flavor. They too argue that mergers between large corporations are unlikely to be based on performance of specific branches or facilities.[38]

Another endogeneity problem we face is an omitted variable bias. We observe empirically that M&A's tend to occur between closely located plants. Then, it is not unreasonable to suspect that two plants that share a similar procurement environment are not only more likely to merge but also more likely to consolidate their suppliers. However, many aspects of procurement environment are often unobserved including shipping costs, the number of suppliers in the choice set, and the degree of environmental compliance. We address a potential omitted variable bias by restricting the sample to be pairs of plants in close proximity that arguably share similar local market conditions. The flavor of this identification strategy is

[37] For mergers, we assign a bigger firm to be an acquiring firm.

[38] Hastings (2004) argues that the acquisition of Thrifty gas stations in California by an oil supplier, ARCO was a decision made as a whole rather than a station-specific decision based on local market conditions. Different mergers have been argued as 'national' mergers in different industries: health insurance (Dafny, Duggan and Ramanarayanan, 2012), banks (Allen, Clark and Houde, 2014), airlines (Kim and Singal, 1993), gasoline (Hastings, 2004; Taylor and Hosken, 2007; Simpson and Taylor, 2008; Houde, 2012)), cement (Hortaçsu and Syverson, 2007) , home appliances (Ashenfelter, Hosken and Weinberg, 2013) and beer (Ashenfelter, Hosken and Weinberg, 2015).

similar to Dranove and Lindrooth (2003) who compares operating costs between merging and non-merging hospitals but limits the non-merging hospitals to be the hospitals that share similar characteristics with the merging ones.

Looking at closely located plant pairs whose mergers are arguably exogenous to a) overarching firm-level M&A decisions and b) unobservable local market conditions, we find that power company M&A's facilitate supplier consolidation. When two power plants become a part of the same network upon a firm level M&A, two power plants become about 23% more likely to source from common suppliers and increase their coal dependence from the common suppliers by 47%.

The rest of the paper proceeds in the following order. Section 2.2 briefly summarizes the institutional setting of the electricity/coal industry and describes our data and presents summary statistics. Section 2.3 describes the relationship between M&A's and procurement decisions. In Section 2.4, we conclude and suggest a direction for future research.

Related Literature

We contribute to two strands of literature: merger and bargaining. First, while the merger literature is vast, there is very little direct evidence on how mergers induce cost reductions.[39] In part, this is because of inherent difficulties in gathering the corporate information related to costs [DOJ & FTC, 2010]. To our knowledge, Ashenfelter, Hosken and Weinberg (2015) is the only paper that directly identifies a cost reduction mechanism of a merger. They find that a merger between two beer manufacturers leads to substantial reductions in shipping costs as the merged firm begins to produce beer in multiple breweries across the U.S. Yet, in

[39] Ashenfelter, Hosken and Weinberg (2014) provides an extensive survey of papers that conduct a retrospective merger analysis. Weinberg and Hosken (2013) provides an extensive survey of papers that use structural models to simulate the impact of mergers on prices and costs.

other industries, researchers often do not directly observe the mechanisms through which a merged buyer extracts more discounts from its suppliers.[40] Although this paper does not directly estimate the impact of supplier consolidation on negotiated input prices, it recognizes supplier consolidation as a potential cost reduction mechanism of mergers.

Second, we visit the assumption of exogenous firm networks that "Nash-in-Nash" bargaining literature tends to leverage. Proposed by Horn and Wolinsky (1988), "Nash-in-Nash" bargaining refers to a Nash equilibrium in a Nash bargaining game (Collard-Wexler, Gowrisankaran and Lee, 2016). In the context of a bargaining model, the Nash equilibrium (i.e., the first "Nash") implies that an upstream and a downstream firm negotiate a price *conditional on all other pairs reach an agreement*. Hence, empirical "Nash-in-Nash" bargaining papers assume that firms do not deviate from their bargaining counter-parties observed in the data. Despite the exogeneity assumption of "Nash-in-Nash" bargaining on firm networks, the bargaining concept has been widely used in the applied literature for its tractability and computational advantage. For example, researchers have employed the protocol in studying negotiated prices between television content providers and cable companies (Crawford and Yurukoglu, 2012; Crawford et al., 2015), hospitals and health care insurers (Gowrisankaran, Nevo and Town, 2015; Ho and Lee, 2017), hospitals and medical device manufacturers (Grennan, 2013), and coffee manufacturers and retail supermarkets (Draganska, Klapper and Villas-Boas, 2010).

However, a growing empirical literature on endogenous network formations challenges the exogeneity assumption on firm networks to be unrealistic (Ghili,

[40] In the health care sector, it is well established that hospital mergers lead to increases in negotiated hospital prices with insurers. Gaynor and Town (2012) provides an extensive summary of papers with such findings. While increased bargaining leverage is argued as a reason behind merger-induced price increases, measures of bargaining leverage are, to large extent, unobserved.

2016; Ho and Lee, 2016; Liebman, 2016; Wang, 2017). If firms are allowed to negotiate with other firms outside of their network, their bargaining leverage tends to increase as the value of their outside option increases. However, while the exogeneity assumption is fundamentally an empirical question, there is very little evidence on how firms choose their suppliers, especially upon mergers.

2.2 Institution & Data

2.2.1 Institution

In theory, the electricity/coal sector exhibits a few features that make a downstream firm's supplier consolidation attractive or at least less harmful. First, coal mining often exhibits a sizable —though heterogeneity is substantial— minimum efficient scale in production (Zimmerman, 1981). Empirically, Boyd (1987) finds that only a handful of mines in Illinois achieved the optimal production scale in the mid 70's. Then, it can be beneficial for a buyer to reduce the number of suppliers and in extreme, commit all of its coal purchase from one large mine (Joskow, 1985). The idea is that once the buyer commits, the mine operator can expand its capacity to an efficient scale, reduce unit costs and potentially pass on some of the savings as a lower price.

Yet, one might argue that larger quantity contracts create a hold-up problem for the buyer. The problem is that a supplier can extract most of the surplus of a buyer when the supplier knows that "the larger the annual quantity of coal that is contracted for, the more difficult it will be for a buyer to replace supplies at a comparable price if the seller withdraws them from the market" (Joskow, 1987*a*). However, by specifying the conditions for future transactions ex ante, long-term contracts can protect the buyer from ex post opportunism problems (Williamson, 1983). Joskow (1987*a*) finds that empirically, the larger the contract quantities are, the longer the contract duration is, suggesting that longer contracts are employed to protect the participants from an increased hold-up problem. Moreover, the nature of a hold-up problem applies to suppliers as well; "the larger the annual quantity of coal that is contracted for, the more difficult it is for the seller to quickly dispose of unanticipated supplies if the buyer breaches" (Joskow, 1987*a*). So, it is unclear whether larger quantity contracts create a more of a severe hold-up problem for the supplier or the buyer. At the least, to the extent that larger quantity contracts improve a supplier's production economies of scale, a merger would incentivize the merged firm to consolidate its coal demand into a handful of large contracts. Nonetheless, whether mergers lead to supplier consolidation remains as an empirical question.

2.2.2 Data Sources

Data used in this paper is constructed by merging multiple databases from multiple data agencies: Energy Information Administration (EIA), Environmental Protection Agency (EPA), S&P Global Platts, and SDC Platinum. Data consists of two major components: fuel transactions between power plants and suppliers, and M&A's and ownership structure of power plants. First, EIA's form 423/923 provide the fuel transaction data. S&P Global Platts' North American Coal Fundamental Dataset supplements the supplier information in the EIA data. Second, EIA's forms 860, 876 and 906, EIA's Electric Monthly Power and EPA's eGrid provide the base for plants' ownership structure. SDC Platinum M&A database supplements the power company M&A information.

2.2.3 Summary Statistics on Supplier Concentration

We provide descriptive evidence that in the electricity/coal sector, supplier concentration is common. First, we show that power plants rely on a small number of suppliers and commit to a very few suppliers. Table 2.1 reports summary statistics on the number of suppliers and the concentration ratios of the largest suppliers. We see that plants on average contract with 3.3 suppliers but commit 73% of its coal delivery from the largest supplier.

Table 2.1: Summary Statistics on Supplier Consolidation

	Mean	Median	S.d.	Obs
Number of Suppliers	3.33	2.00	2.81	8199
CR1	0.73	0.76	0.25	8197
CR2	0.90	1.00	0.15	8197
CR3	0.95	1.00	0.10	8197

Note: Unit of observation is a plant-year.

Second, we show that plants have an access to multiple suppliers but still rely on a relatively small number of suppliers. In principle, larger plants can allocate its coal demand to more suppliers. In Table 2.2, we report the same statistics in Table 2.1 by four different quantiles of plant capacity. We do observe that larger plants tend to have more suppliers. For example, plants with their capacity in the 4^{th} quantile (i.e., Column 4) have about 4.9 suppliers on average while plants with their capacity in the 1^{st} quantile (i.e., Column 1) have about 2.2 suppliers. Nonetheless, larger plants also commit most of the delivery to a few largest suppliers. Plants with the capacity in the 4^{th} quantile (i.e., Column 4) source about 64% of its delivery to the largest supplier and 83% from the two largest suppliers.

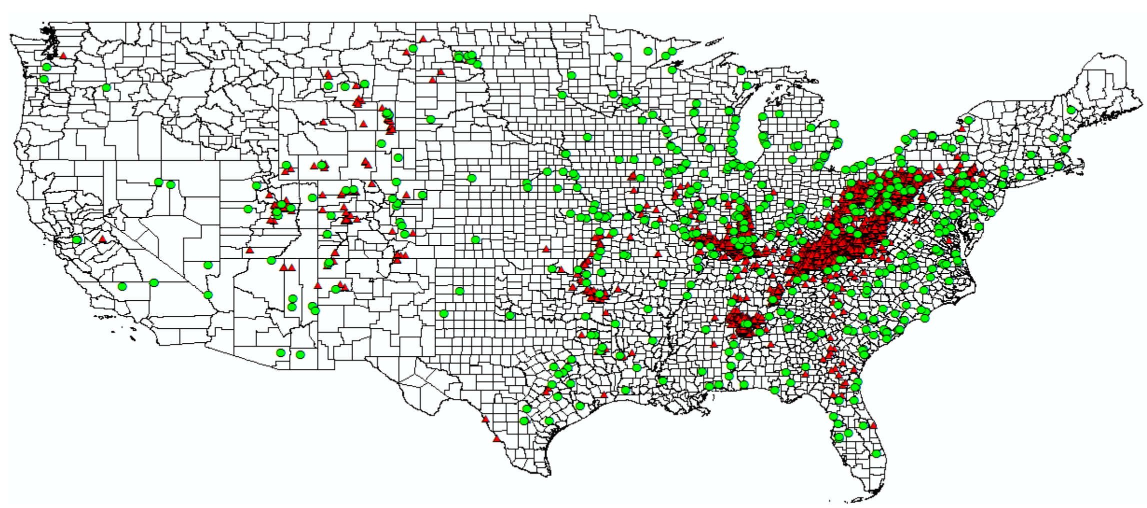
Table 2.2: Summary Statistics on Supplier Consolidation by Plant Size

	Q1	Q2	Q3	Q4
Capacity (MW)	192.22	525.33	996.94	2026.08
Number of Suppliers	2.17	2.73	3.53	4.92
CR1	0.83	0.77	0.69	0.64
CR2	0.95	0.92	0.87	0.83
CR3	0.98	0.97	0.94	0.91
N	2067	2044	2051	2035

Note: Each column denotes four different quantiles of plant capacity measured in megawatts.

Graphically, Figure 2.2.1 shows a distribution of power plants and coal mines in the U.S. Although suppliers tend to own multiple mines, the map suggests that across the country except for the West, power plants have a large reservoir of suppliers from which they can source their coal.

Figure 2.2.1: Coal-fired Power Plants and Coal Mines in the U.S.

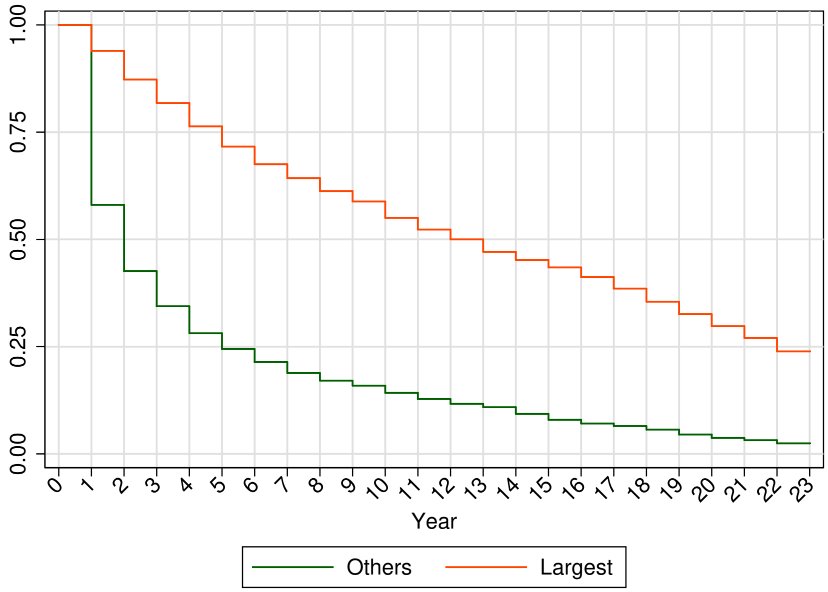


Note: Green circles represent coal-fired power plants. Red triangles represent coal mines. An establishment is shown if it ever operated between 1990 and 2012.

Lastly, we show that larger suppliers tend to engage in longer contracts. In theory, long-term contracts can mitigate a hold-up problem induced by larger quantity contracts. Then, we expect to observe longer contracts for larger suppliers.

Figure 2.2.2 plots a fraction of suppliers that "survive" or continue to deliver to a plant from $t-1$ to t. The graph plots the survival rate for the largest suppliers and the rest of the suppliers where a supplier is plant-specific. That is, in the survival analysis, we regard a supplier A to a plant 1 as a separate entity from the same supplier A to another plant 2. A plant-specific supplier is defined to be the largest supplier if it is ever the largest supplier to that particular plant. For example, the largest supplier to a plant A is not necessarily the largest supplier to a plant B. The graph shows that after about 4 years of coal delivery, about 75% of the largest suppliers continue their delivery while only about 25% of all other suppliers continue to do so. This empirical observation neither proves or disproves the theory that longer contracts mitigate the hold-up problems. Nevertheless, because long-term contracts are otherwise undesirable (Joskow, 1988), the positive correlation between contract quantities and contract length seems to suggest that long-term contracts are employed to address the potential hold-up problems.

Figure 2.2.2: Duration of Plant-Supplier Relationship



Note: Kaplan-Meier survival probability estimates. Unit of observation is a plant-supplier-year. A supplier is defined to be the largest supplier if it is ever the largest supplier.

2.2.4 Summary Statistics on M&A Activities

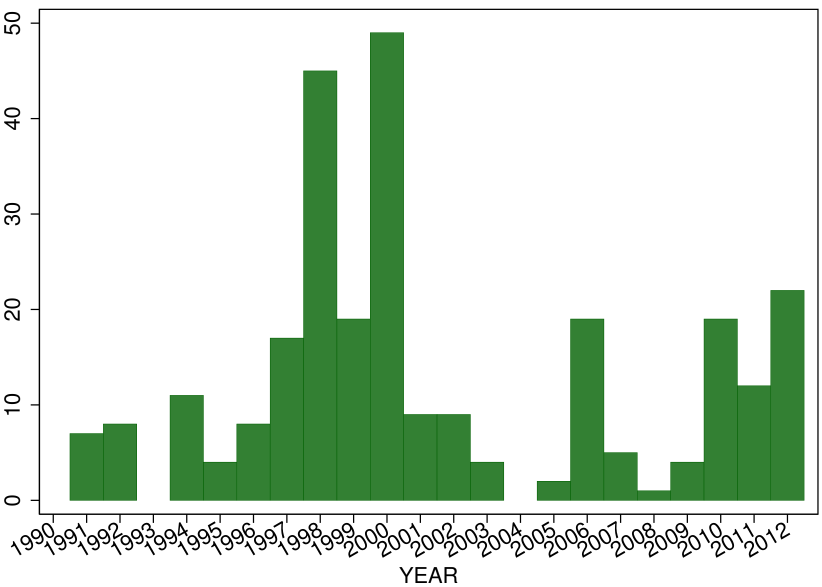
In this subsection, we present the aggregate trends in M&A's and show that M&A's are associated with more supplier switching, a precursor of supplier consolidation.

In the electricity sector, there are a total of 73 unique M&A's between power companies from 1990 to 2012. These 73 M&A's involve a total of 274 (188 unique) plants. On average, each M&A changes the ownership structure of about 5.3 plants. About 50 plants go through multiple M&A's. Sub-figure (a) of Figure 2.2.3 shows that the number of M&A's peaks between 1998 and 2000 but M&A's do occur throughout the entire period. Sub-figure (b) of Figure 2.2.3 shows that the number of power companies decline over time as a result of the M&A's. Yet, the number of power plants is mostly stable over time until 2012. Excluding the year

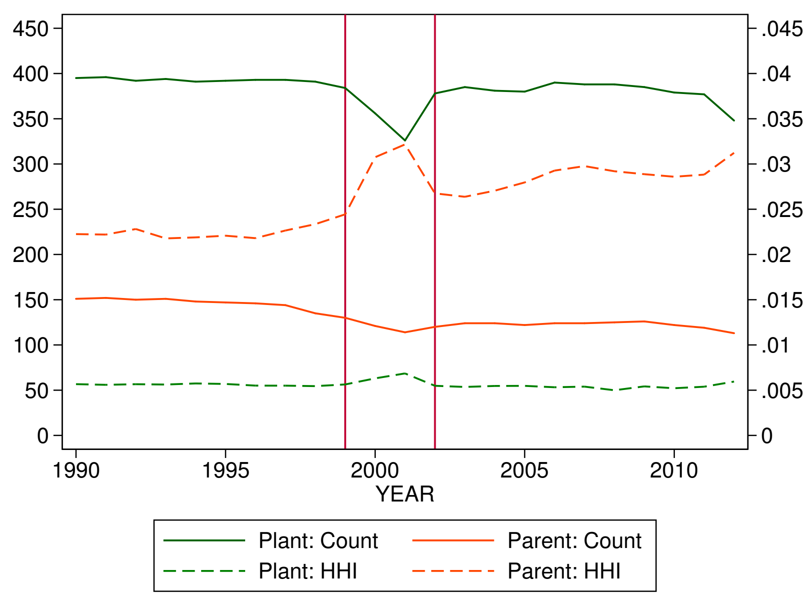
2012, there is, on average, 1 entry and 1.5 exits per year throughout the period.[41]

Figure 2.2.3: Aggregate Trends

(a) M&A



(b) Power Plant, Parent Company, and HHI



[41] About 20 exits happen in 2012 alone.

In Table 2.3, we summarize the characteristics (i.e., capacity and distance) of M&A's from an individual plant's perspective. At the plant level, an average M&A brings about additional 4400 MW capacity to the plant's original network. This new addition is considerable in size given that the original network has about 5000 MW of capacity on average. Moreover, M&A's are between closely located companies. Again from a plant's perspective, the shortest distance to a plant in the merging network is about 305 km. Relatively to the minimum distance to the original network (i.e., the shortest distance within the same network), which is about 80 km, the distance to the merging network is relatively far away. However, given that the average distance between any two random plants in the same census division is about 800 km, M&A's still tend to occur between firms in proximity. Lastly, a typical M&A involves a group of 6.4 plants merging with or acquiring a group of 4.3 plants.

Table 2.3: Summary Statistics on Mergers and Acquisitions

	Mean	Median	S.d.	Obs
A. Plant level				
Min. Dist. to Original Network (km)	80.47	43.62	122.18	274
Min. Dist. to Merging Network (km)	304.66	157.95	401.48	274
Original Network Capa. (GW)	5.05	5.60	3.03	274
Merging Network Capa. (GW)	4.42	3.33	4.88	274
B. M&A level				
Number of Plants: Original Network	6.36	5.00	5.34	73
Number of Plants: Merging Network	4.31	3.00	3.42	73

Note: For Panel A., unit of observation is a plant-year. For Panel B, unit of observation is a firm-level M&A.

2.2.5 M&A and Supplier Switching

Now, we explore whether M&A's are associated with more supplier switching. In particular, we regress a dummy indicator for whether a plant switches its suppliers on a dummy indicator for whether a plant is involved in a M&A in the last three years. It is important to note that such regressions show correlations rather than causality and that supplier switching is not a sufficient condition for supplier consolidation. Nonetheless, the regressions are useful in that they shed some light on the issue of omitted variable bias. Formally, we estimate:

$$\mathbf{1}(Switch\,Supplier)_{it} = \alpha \cdot \mathbf{1}(M\&A\;Last\,3\,Years)_{it} + X_{it}'\beta + \gamma_t + \epsilon_{it}$$

where $\mathbf{1}(Switch\,Supplier)_{it}$ is defined to be 1 if the plant i switches its largest supplier in time period t from $t-1$, $\mathbf{1}(M\&A\;Last\,3\,Years)_{it}$ is defined to be 1 if the plant i is involved in a M&A in any time period from $t-3$ to t, X_{it} denotes a set of plant characteristics and γ_t denotes either year or division-year fixed effects where a division is a census division. Intuitively, the division-year effects allow the M&A dummy to capture the mean difference in the probability of supplier switching across the M&A plants and the non-M&A plants in the same census division and time period. This specification is useful in that it compares plants that are facing the same division-level procurement environment, which the baseline regression with the year effects do not control for.

Table 2.4 reports the estimation results with the outcome variable defined based on the largest supplier. Column (1) is a baseline specification with the year effects and without any controls. The coefficient represents that the probability of switching the largest supplier is correlated with the plants that are a part of a M&A in the last three years by 3.6 pp more than it is with the plants that are not a part of a M&A in the last three years. Column (2) accounts for some plant characteristics. The inclusion of plant characteristics removes the statistical significance, but the magnitude of the coefficient remains mostly unchanged. However, plant characteristics do not fully control for unobservable procurement conditions that the plant might face. Column (3) accounts for any division-specific market conditions by employing the division-year fixed effects instead of the year fixed effects. The coefficient shows that conditional on the division level market conditions, the probability of switching the largest supplier is correlated with the M&A plants by 5.8 pp more than the non-M&A plants. Since about 25% of the M&A plants switch their largest suppliers, the coefficient of 5.8 pp translates into about 23% increase in the probability of switching.

Yet, all of the regressions in Table 2.4 do not specify a plant fixed effects because the outcome variable is defined inter-temporally. Because M&A plants can be systematically different from the non-M&A plants in terms of supplier switching, we also estimate a specification (i.e., Column 4) only with the plants that is ever a part of a M&A. In this specification, the M&A coefficient would be identified by the within plant variation by whether or not that particular plant is a part of a M&A in the last three years. Column (4) reports the estimation result. The estimated coefficient in Column (4) remains unchanged from the one in Column (3), suggesting that the probability of supplier switching in Column (3) is not entirely identified from the level difference between the M&A plants and the non-M&A plants. Lastly, Column (5) breaks down the effect of a M&A on supplier switching into individual years since the M&A. Column (5) suggests that supplier switching can take place any time between the year of M&A to 4 years after the M&A, but diminishes after 5 years of the M&A. As a robustness check, we repeat the exercise with the outcome variable based on the three largest suppliers. The estimated coefficients (not reported in the paper) are similar to the coefficients in Table 2.4.

Table 2.4: Estimates of Supplier Switching

	(1)	(2)	(3)	(4)	(5)
1(M&A last 3 yrs)	0.036*	0.033	0.058***	0.052**	
	(0.022)	(0.021)	(0.020)	(0.022)	
1(Yr of the M&A)					0.043
					(0.031)
1(1 yr after M&A)					0.063*
					(0.032)
1(2 yrs after M&A)					0.059*
					(0.035)
1(3 yrs after M&A)					0.073*
					(0.038)
1(4 yrs after M&A)					0.034
					(0.035)
1(5 or more yrs after M&A)					-0.007
					(0.021)
Controls		X	X	X	X
Year FE	X	X			
Division-Year FE			X	X	X
Plant FE					
R^2	0.008	0.043	0.108	0.142	0.108
Total Plants	410	409	409	188	409
Obs.	7583	7578	7578	3498	7578

Notes: The outcome variable is a dummy indicator for a switch in the largest supplier from $t-1$ to t. A set of control variables include a dummy for a coal type switch from bituminous coal in time $t-1$ to sub-bituminous coal in time t, nameplate capacity measured in megawatts, number of combustion generators, number of steam generators, and average shipping distance of coal in time $t-1$. Standard errors are clustered at the plant level.

We then explore whether supplier switching is more likely if merging plants are closer to the network being acquired. If plants that are closer to a merging network switch their suppliers more than the plants that are distant from the merging network, then it is more difficult to rule out an omitted variable bias from unobservable procurement conditions. For example, if unobservable shipping costs are not an important factor in sourcing decisions, then plants with different distances to a merging network should unilaterally switch their suppliers. However, we find that plants with different distance respond differentially. Formally, we test whether a plant's distance to a merging network has a heterogeneous impact on supplier switching. We regress supplier switching on a M&A dummy and a M&A dummy interacted with the minimum distance to a merging network.

Table 2.5 show the estimation results. In Column (1), the negative coefficient on the interaction term suggests that merging plants distant from the merging network switch their suppliers at a lower rate than the merging plants closer to the merging network. If division-specific trends are controlled for (i.e., Column 2), the magnitude of the coefficient on the interacted term becomes smaller. However, it is still negative and statistically different from 0, suggesting that procurement conditions in a local market (smaller than the census division market) can be driving the probability of switching. In Columns (3) and (4), a discrete measure of the M&A distance is used: a dummy indicator for the plants with the above median value of the shortest distance to a merging network. Column (3) uses the year effects and Column (4) uses the division-year effects. Though the coefficient on the interaction term becomes statistically insignificant in Column (4), a sizable point estimate still suggests that unobservable market conditions are a threat to the identification.

Table 2.5: Estimates of Supplier Switching: By Plant Distance to M&A

	(1)	(2)	(3)	(4)
1(M&A last 3 yrs)	0.101***	0.105***	0.072**	0.075***
	(0.035)	(0.033)	(0.029)	(0.028)
1(M&A last 3 yrs)· log(Min. dist.)	-0.017**	-0.012*		
	(0.007)	(0.006)		
1(M&A last 3 yrs)· 1(min. dist. above median)			-0.078**	-0.038
			(0.039)	(0.036)
Controls	X	X	X	X
Year FE	X		X	
Division-Year FE		X		X
Plant FE				
R^2	0.044	0.108	0.044	0.108
Total Plants	409	409	409	409
Obs.	7578	7578	7578	7578

Notes: The outcome variable is a dummy indicator for a switch in the largest supplier from $t-1$ to t. log(Min. dist.) is the log of the shortest distance between a merging plant and any of the plants in the network being acquired. 1(min. dist. above media) is an indicator variable equal to 1 if the shortest distance to merging network is above the median. A set of control variables include a dummy indicator for a coal type switch from bituminous coal in time $t-1$ to sub-bituminous coal in time t, nameplate capacity measured in megawatts, number of combustion generators, number of steam generators, and average shipping distance of coal in time $t-1$. Standard errors are clustered at the plant level.

2.3 Mergers & Sourcing Decisions

In this section, we estimate the impact of M&A's on supplier consolidation. We transform our data from a plant level to a plant-plant pair level and restrict our sample to be pairs of closely located plants to establish causality. We first explain why we shift to a pair level analysis. Naturally, whether or not plants combine or consolidate their suppliers is a joint decision involving at least two entities. Uni-entity or a plant level measures of supplier consolidation such as a number of suppliers or a HHI measure can be misleading because the plant level measures do not fully capture a plant's supplier consolidation. For example, consider a plant that sources its coal from a single supplier. Now, the plant shifts its entire coal demand to a different supplier which is already shipping to another plant owned by the same parent company. Then, the two plants have combined or consolidated their suppliers. However, from the original plant's perspective, neither the number of suppliers nor the HHI measure changes. In order to avoid this problem, we transform the data from a plant-year level to a pair-year level unit of observation.

In the pair level data, measures of supplier consolidation are defined naturally. We study whether a pair shares common suppliers and how much of coal is delivered from the common suppliers. In particular, we define the following variables: $1(Any\, Common)$ is a dummy indicator for whether a pair shares any common suppliers, $1(L1\, Common)$ is a dummy indicator for whether a pair shares the respective largest supplier as a common supplier, $1(L3\, Common)$ is a dummy indicator for whether a pair shares any of the three respective largest suppliers as a common supplier, $\#(Total)$ is the total number of suppliers by a pair, HHI is the Herfindahl-Hirschman Index of suppliers delivering to a pair, $\#(Common)$ is the number of common suppliers shared by a pair, $F\, Common$ is a fraction of the total coal quantities delivered by the common suppliers, $F\, L1\, Common$ is a fraction of the total coal quantities delivered from the largest common supplier, and $F\, L3\, Common$ is a fraction of the total coal quantities delivered from the three largest common suppliers. We, then, test:

$$y_{it} = \alpha \cdot \mathbf{1}(M\&A\, yet)_{it} + X_{it}'\beta + \gamma_i + \gamma_t + \epsilon_{it}$$

where i denotes a pair of two plants, y_{it} can be any of the afore-mentioned measures of supplier consolidation, $\mathbf{1}(M\&A\, yet)_{it}$ is a dummy variable equal to 1 if the pair i undergoes a M&A and t is a time period after a M&A, X_{it} measures similarities in the plant characteristics between the two plants of the pair i, γ_i denotes the pair fixed effects and γ_t denotes the time fixed effects.

We remind the reader that testing whether M&A's lead to supplier consolidation suffers from two endogeneity problems. First is a selection bias that mergers are not random but are choices by firms. We have argued that performance of any particular pair of two plants from the merging firms is orthogonal to the firm-level M&A decision. Another endogeneity problem is unobservable procurement environment. We address the omitted variable bias more directly by looking at pairs of plants in close proximity that share similar local market conditions. In particular, we restrict the sample to the plant pairs with the pair distance less than 300km. In a given census division, an average distance between any two random plants is about 800km.

A pair level analysis is first implemented on the dummy measures of supplier consolidation. Table 2.6 reports the estimation results. The outcome variables are $1(Any\,Common)$ in Columns (1) and (2), $1(L1\,Common)$ in Columns (3) and (4), and $1(L3\,Common)$ in Columns (5) and (6). In the odd number columns, there is no restriction on the pair distance. The even number columns restrict the sample to be the pairs of plants that are within 300km. Across the odd number columns compared to their even column counterparts, the coefficients are biased upwards. The difference between the odd and the even columns suggests that restricting the sample to be the pairs of closely located plants accounts for some of the local unobservables. In the even columns, the coefficients are stabilized and generally lower. Nonetheless, the results still suggest that upon a M&A, plant pairs start sourcing from common suppliers. In particular, the estimated coefficient in Column (2) implies that upon a M&A, two merged plants is 15.4 pp more likely to share a common supplier. Since the fraction of pairs that share any common supplier is about

66.8%, 7.7 pp translates into about a 23% effect. Columns (4) and (6) respectively suggest that about 51% (0.079/0.154) and 97% (0.149/0.154) of the overall M&A effect in Column (1) are due to pairs consolidating their largest supplier and any of the three largest suppliers.

Table 2.6: Estimates of Supplier Consolidation

	(1) 1(Common)	(2) 1(Common)	(3) 1(L1 Common)	(4) 1(L1 Common)	(5) 1(L3 Common)	(6) 1(L3 Common)
1(M&A yet)	0.169***	0.154***	0.080***	0.079***	0.151***	0.149***
	(0.017)	(0.018)	(0.011)	(0.012)	(0.018)	(0.019)
Base	0.550	0.668	0.269	0.338	0.449	0.571
Controls	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Pair FE	X	X	X	X	X	X
Pair Dist.		300		300		300
Frac. M&A Pairs	0.016	0.117	0.016	0.117	0.016	0.117
Total Pairs	85052	6194	85052	6194	85052	6194
R^2	0.156	0.221	0.039	0.085	0.113	0.162
Obs.	1447742	102522	1447742	102522	1447742	102522

Notes: A set of control variables include percentage difference in nameplate capacity between the two plants of a pair, percentage difference in the average shipping distance to coal, difference in the number of combustion generators, difference in the number of steam generators and a dummy indicator for whether two plants share a common coal type. Standard errors are clustered at the plant level.

The estimation results are not sensitive to the choices of the pair distance thresholds. Table 2.7 reports estimation results with different pair distance thresholds.

Table 2.7: Estimates of Supplier Consolidation: Robustness

	(1) 1(Common)	(2) 1(Common)	(3) 1(Common)	(4) 1(L1 Common)	(5) 1(L1 Common)	(6) 1(L1 Common)
1(M&A yet)	0.151***	0.154***	0.173***	0.080***	0.079***	0.075***
	(0.016)	(0.018)	(0.021)	(0.012)	(0.012)	(0.016)
Base	0.652	0.668	0.707	0.329	0.338	0.359
Controls	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Pair FE	X	X	X	X	X	X
Pair Dist.	400	300	200	400	300	200
Frac. M&A Pairs	0.077	0.117	0.161	0.077	0.117	0.161
Total Pairs	10281	6194	3073	10281	6194	3073
R^2	0.209	0.221	0.241	0.074	0.085	0.115
Obs.	170739	102522	51333	170739	102522	51333

Notes: A set of control variables include percentage difference in nameplate capacity between the two plants of a pair, percentage difference in the average shipping distance to coal, difference in the number of combustion generators, difference in the number of steam generators and a dummy indicator for whether two plants share a common coal type. Standard errors are clustered at the plant level.

Next, we look at the magnitude of supplier consolidation. The outcome variables are $\#(Total)$, $\#(Common)$, HHI, $F\,Common$, $F\,L1\,Common$ and $F\,L3\,Common$. Table 2.8 reports the estimation results with the 300km threshold. They suggest that upon a M&A, a plant pair combines its supplier base in two different ways. First, a plant pair can together find an entirely new common supplier. Second, one of the plants of a pair can discontinue its relationships with the existing suppliers and switch to the other plant's suppliers. Yet, regardless of a consolidation mechanism a plant pair employs, the results suggest that plant pairs increase their dependence on the common suppliers.

The estimated coefficient in Column (1) shows that a merger induces a plant pair to increase the total number of suppliers by 0.256 or about 4% compared to the baseline count of 7. At first, this result seems to suggest that plant pairs are expanding rather than consolidating their supplier base. However, Column (2) reveals that the number of common suppliers outgrows the number of total suppliers. That is, even if one assumes that 0.256 out of the 0.591 increase in the number of common suppliers is from finding a new common supplier, there still is an increase in the number of common suppliers by 0.335 (0.591-0.256). This suggests that at least one of the plants of a pair replaces its existing suppliers with the other plant's suppliers. If there is no supplier consolidation (i.e., removing and combining suppliers), then an increase in the number of common suppliers can only arise from increasing the number of entirely new common suppliers and thereby cannot be greater than an increase in the number of total suppliers.

Yet, regardless of how plant pairs start to source from common suppliers, M&A's lead the plant pairs to increase their dependence on the common suppliers. While Column (3) shows that HHI decreases upon a M&A —a consequence foreshadowed by the increase in the total number of suppliers—, the estimated coefficient in Column (4) reveals that the fraction of coal delivered from the common suppliers increases by 0.071 pp or by 47% from the baseline fraction of 0.15. Columns (5) and (6) show that about 39% (0.028/0.071) and 80% (0.057/0.071) of the overall increase in the fraction is attributed to the largest and the three largest suppliers, respectively.

Table 2.8: Estimates of Supplier Consolidation: Continuous Measures

	(1) #(Total)	(2) #(Common)	(3) HHI	(4) F Common	(5) F L1 Common	(6) F L3 Common
1(M&A yet)	0.256**	0.591***	-157.391**	0.071***	0.028***	0.057***
	(0.125)	(0.075)	(69.468)	(0.009)	(0.005)	(0.008)
Base	7.002	1.179	5277.295	0.150	0.053	0.118
Controls	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Pair FE	X	X	X	X	X	X
Pair Dist.	300	300	300	300	300	300
Frac. M&A Pairs	0.117	0.117	0.117	0.117	0.117	0.117
Total Pairs	6194	6194	6194	6194	6194	6194
R^2	0.527	0.268	0.550	0.200	0.109	0.176
Obs.	102522	102522	102522	102522	102522	102522

Notes: A set of control variables include percentage difference in nameplate capacity between the two plants of a pair, percentage difference in the average shipping distance to coal, difference in the number of combustion generators, difference in the number of steam generators and a dummy indicator for whether two plants share a common coal type. Standard errors are clustered at the plant level.

2.4 Conclusions and Future Research

Using the unique transaction data between coal-fired power plants and coal suppliers in the U.S., this paper provides the first empirical evidence on whether mergers induce firms to consolidate their supplier base. Specifically, we find that merged plant pairs become about 23% more likely to share suppliers. About a half of the effect arises from consolidating the largest supplier. We also find that upon a M&A, plant pairs receive a greater fraction of coal from the common suppliers. The share of coal delivered by the common suppliers increases by 47%.

However, our results compel more careful analysis to follow. One immediate worry is that while we argue that a particular plant pair is less likely to be instrumental to a firm level M&A decision, a selection bias can indeed persist at the pair level. First, certain plants may be the crown and jewels of mergers. Also, if each of the merging firms is very closely clustered to the extent that differences (e.g., distance) in any random pairs of the merging firms are minimal, then a firm-level merger is effectively a pair-level merger. Future analysis can address this issue by matching merging plant pairs to non-merging plant pairs that are hypothetically more likely to merge (Dranove and Lindrooth, 2003). Another issue with the regression analysis in this paper is that for the plant pairs that undergo multiple M&A's, the first M&A is assumed to capture all the potential impact of subsequent M&A's on supplier consolidation. Given that about a quarter of plants that are ever a part of a M&A goes through multiple M&A's, our analysis is likely to be biased upwards. Yet, one can estimate a M&A-specific effect by interacting the M&A term with a dummy indicator for individual M&A's. Then, the effects of multiple M&A's can be separately identified. Lastly, future research should also explore the impact of a M&A on negotiated prices between power producers and coal suppliers. Increased bargaining leverage from supplier consolidation should obtain lower rates for the power companies, at least in theory. Empirical estimation of the magnitude of the price discounts from supplier consolidation will add a dialogue to a controversial idea in antitrust policies that downstream mergers should be permitted to counterbalance the upstream market power.[42]

[42]Thomassen et al. (forthcoming) discusses that while mergers would increase the market power of supermarkets, they can also help the supermarkets negotiate better prices with wholesalers or manufacturers.

Appendix

A.1 Institutional Overview

A.1.1 Regulatory Structure and Power Producer Typology

Historically, the US power industry has been regulated on the federal and the state level. The Federal Energy Regulatory Commission (FERC) regulates interstate transmission and wholesale power sales, while the Public Utility Commissions or Public Service Comissions (PUCs/PSCs) are the regulatory authorities overlooking retail sales of electricity, construction of transmission lines within their boundaries, and intrastate distribution.

In the 1990's, the federal government started restructuring the wholesale electricity market by unbundling generation from transmission and distribution. In 1992, the Energy Policy Act (EPACT) oversaw a radical overhaul to the industry structure. The FERC issued Order No. 636, known as the Restructuring Rule, which mandated open access to transmission system and separation of electricity sales from transportation services. Although the implementation of the EPACT was up to state's discretion, a number of states started restructuring its regional electricity market. The FERC further promoted competition through a consequent series of orders; it mandated non-discriminatory pricing and access to transmission services –Order No.888 in 1996–, and established definitions of voluntary, non-profit organizations –Order No. 2000 in 1999– that would manage the wholesale electricity markets. These entities called Regional Transmission Organizations (RTOs) and Independent System Operators (ISOs) have taken more roles over time; they provide an fair access to the regional transmission infrastructure and serve as a wholesale market in which buyers and sellers could bid for or offer generation. As of 2014, 7 RTOs/ISOs serve about roughly two-thirds of transmission in the U.S. The rest of electricity is served in the traditional market where vertically integrated utilities are usually responsible for both generation and transmission.

In the late 90's as the electricity industry was restructured, non-utilities started to grow. Unlike the traditional utilities who were responsible for generation, transmission and distribution, the non-utilities focused only on generation. The Energy Information Administration (EIA) classifies non-utilities into 3 types: Independent Power Producers (IPPs), commercial and industrial non-utilities. While the last two non-utilities produce electricity primarily for their own use, an IPP operates merchant power plants and sells electricity on the wholesale market or via power purchase agreements.

A regulated electric utility, by contrast, operates power plants subject to a rate-of-return regulation and directly serves customers within its service territory. A PUC determines an allowed return on capital for each utility during a rate hearing, and the utility sets its retail electricity prices accordingly. Rate hearings are often costly and hence held intermittently. In the past, this implied that the power utilities were vulnerable to risks from volatile changes in the fuel prices because retail price adjustments were inflexible. By 1979, as most of the PUCs adopted the Fuel Adjustment Clauses (FAC), the utilities were able to pass through the changes in fuel costs directly on to the consumer without the need of a rate hearing (Knittel, Metaxoglou and Trindade, 2014). However, the FACs also created the disincentives as the utilities did not have to bear the burden from excessive fuel use (Brown, Einhorn and Vogelsang, 1991). Since then, the PUCs have modified the FACs overtime such that the firms absorb a portion of fuel cost overruns, as well as profit from lower than expected fuel costs (Knittel, 2002*b*). Currently, most of states have the FAC such that it becomes effective only when the fuel costs surge above or fall below certain thresholds. In such case, the utilities become the residual claimant to reductions in input costs up to the automatic adjustment threshold (Joskow, 1974).

A.1.2 Environmental Policies

In the 2000's, federal regulation has focused on setting tighter environmental standards. In 2000, under the Clean Air Act from 1990, the Acid Rain Program (ARP) went into the Phase II, which tightened the annual SO_2 and NO_X emissions limits imposed on the Phase I plants and also set restrictions on smaller, cleaner plants fired by coal, oil, and gas. The program affected existing utility units serving generators with an output capacity of greater than 25 megawatts and all new utility units (EPA, 2012*a*). To accomplish the emissions reductions, the ARP introduced an allowance trading system.

In 2005, the EPA issued the Clean Air Interstate Rule (CAIR). The rule was set to reduce SO_2 and NO_X by 70%. The CAIR covered 27 eastern states and the District of Columbia, and the states were to comply by "(1) meeting the state's emission budget by requiring power plants to participate in an EPA-administered interstate cap and trade system that caps emissions in two stages, or (2) meeting an individual state emissions budget through measures of the state's choosing" (EPA, 2014). The D.C Court vacated the CAIR in 2008 but ordered to hold the CAIR in place until the EPA establishes a replacement.

Seasonal emission programs started well before 2000. In the 2000's, these programs further tightened the emission standards. In 2003, the NO_X Budget Trading Program under the NO_X State Implementation Plan (NO_X SIP) superseded the Ozone Transport Commission (OTC) NO_X Budget Program. In 2009, CAIR's NOx ozone season program under Clean Air Interstate Rule began, effectively replacing the NO_X Budget Trading Program and achieving further summertime NOx reductions from the power sector. These seasonal NO_X programs have targeted 20 Eastern states between May and September of each year (EPA, 2012*b*).

These new environmental policies have impacted the use of coal relatively more than that of natural gas. Historically, in the absence of environmental regulation, coal has been a favored fuel source due to its low relative price. Power producers burn natural gas "as a cleaner-burning fuel in preference to other fossil fuels, to comply with environmental regulations" (Tuthill, 2008).

A.1.3 Background on Coal and Natural Gas Generation

Coal and natural gas have distinctive characteristics that make one commodity more attractive than the other depending on generation technology, cycling costs[43], associated environmental pollution, and the relative outlook of the future fuel prices.

On a generator level, a fuel choice is limited by generator types and cycling costs. There are two major types of generators: steam turbine and combustion turbine.[44] While a steam generator can be operated on any fuel and is often more effi-

[43]Costs associated with ramping up and down power plants

[44]Steam turbine: a device that converts high-pressure steam, produced in a boiler, into mechanical energy that can then be used to produce electricity by forcing blades in a cylinder to rotate and turn a generator shaft. Combustion turbine: an internal combustion engine has one or more cylinders in which the process of combustion takes place, converting energy released from the rapid

cient than a combustion generator, it is associated with more cycling costs. Meanwhile, the combustion turbines are used specifically for natural gas and have low cycling costs. But, they are less efficient. Hence, natural gas power plants are typically operated to meet peak electricity demand while power plants with steam turbines –mostly use coal because of its cheap relative price compared to other fuels– are operated on a continuous basis to serve base load demand.(Dahl and Ko, 2001; Hinrichs and Kleinbach, 2002) Normally, demand load tends to be lowest at night, when most people are asleep, and highest during the day, when most electric appliances are in use. Utilities meet this load by operating a selected subset of all the generators calculated to be the most efficient for the season and time (EIA, 2012).

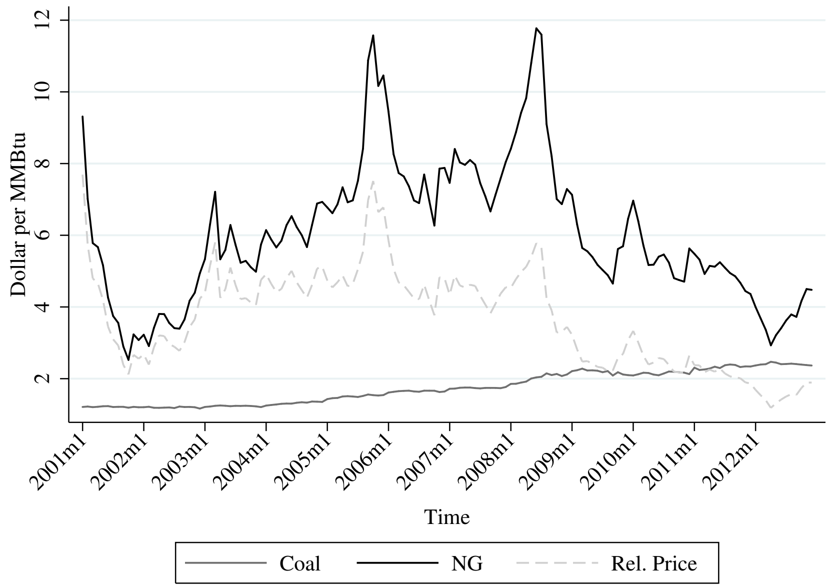
In the era of heavy environmental regulation and shale gas boom, natural gas has an advantage over coal for power generation. Burning natural gas results in much fewer emissions of nearly all types of air pollutants and carbon dioxide per unit of heat produced than coal.[45] Natural gas is also becoming more abundant as recent shale gas development has accelerated with the new applications of hydraulic fracturing technology and horizontal drilling. The total national gas production grew over 25% from 2006 to 2012. Although it is unclear how much the decrease in the energy demand from the 2008 recession affected the natural gas prices, the increased natural gas supply has been identified as one of the major contributor to the lower gas prices since 2008 (EIA, 2014). Delivered coal costs to the electric sector are on an increasing trend due to the growing fuel surcharges added by transportation companies.(EIA, 2013*a*; Knittel, Metaxoglou and Trindade, 2014) Figure 2.4.1 shows that the relative price of natural gas to coal is on a decreasing

burning of a fuel-air mixture into mechanical energy. Source: EIA glossary

[45] About 117 pounds of carbon dioxide are produced per million Btu equivalent of natural gas compared to over 200 pounds of CO_2 per million Btu of coal (EIA, 2013*b*).

trend since 2005.

Figure 2.4.1: Trends in Fuel Prices



However, despite the price advantage of natural gas, utilities are often stuck with burning coal due to long-term coal contracts. Joskow (1987*b*) and Cicala (2015) document that "the market for coal is largely conducted through long-term bilateral contracts." A typical contract lasts from a few months to several years, and firms are locked in with coal until contracts expire.

A.2 Data

A.2.1 Data Sources

Most of the data analyzed in this dissertation are from the Energy Information Administration (EIA) and the Environmental Protection Agency (EPA). EIA-423/923 (previously collected as FERC-423, EIA-906/920) forms provide data on generation and fuel transaction. The plants responsible for reporting are those with generating capacity of 50 MW or more. Form EIA-860 provides generator-level specific information about existing and planned generators and associated environmental equipment at electric power plants with 1 megawatt or greater of combined nameplate capacity. Starting in 2010, the EIA 860 provides information whether a plant is a member of the wholesale market. For the earlier years, I extrapolate the 2010 status; if a plant is connected to an ISO/RTO in 2010, it is assumed that the plant has been since 2001.

The EPA's Air Markets Program Data provides generator-level information about the environmental programs. The programs include the Acid Rain Program, the Clean Air Interstate Rule program, and the various ozone programs like the OTC Program, the NO_X Budget Program and the SIP NO_X Program. This data is provided on an annual level.

The Form EIA-826, "Monthly Electric Utility Sales and Revenue Report with State Distributions," provides data on retail sales of electricity and associated revenue, each month, from a statistically chosen sample of electric utilities in the United States. The state-level retail electricity price data is obtained from the EIA 826. The state-level regulatory status data are hand collected from the EIA's website. If a state has implemented restructuring in many steps, I consider the first month of the implementation as the restructured date. The spot price data and the weather data are obtained from Bloomberg. The state-level GDP per capita and the state-level unemployment rates are obtained from Bureau of Economic Analysis and Bureau of Labor Statistics, respectively.

When all the above data are merged, a good number of mismatches exist. There are 2 main sources of the mismatches. First, the fuel consumption data is collected for the plants with the capacity 50 MW or more. Hence, in this paper I limit the scope of analysis to these plants. Second, prior to 2008 when the FERC collected the fuel receipt and cost data, it issued waivers to the filing requirements to some plants who met certain criteria, and as a result a significant number of plants either did not submit fossil fuel receipts data or submitted partial information.[46] After 2008, the EIA 923's monthly data do not survey all the plants that meet the 50 MW requirement. However, according to the EIA, the monthly sample represents most of the total net generation at approximately 94%. I imputed the missing prices using flexible interactions between the spot prices and the plant-level characteristics. The R-squared for the imputation regressions are over 0.9 for both natural gas and coal. However, missing price data for non-utilities are not imputed. EIA regards the entire price data for non-utilites as business sensitive and do not release the data for public use.

Other data sources include S&P Global Platts, and SDC Platinum. Platts database provides information on the transactions between power plants and suppliers. SDC Platinum databse provides information on the ownership structure of power plants. Various SEC 10-k filing reports are used to supplement the ownership data. In a case that a power plant is owned by multiple entities, the major stake holder is assumed to be the parent company.

A.2.2 Summary Statistics

Table 2.9 and Table 2.10 report various statistics of the power plant characteristics and the state-level market characteristics. Variables defnitions are: *Utility* is the regulated utility sector. *IPP* represents the non-regulated independent power producers. *Ind/Com* is the industrial or the commercial sector. *Coal* is the coal de-

[46]Look at Appendix C, Technical Notes, to the Electric Power Monthly for further details. (`http://www.eia.gov/electricity/monthly/pdf/technotes.pdf`)

manded in billion British thermal unit (Btu). *NG* is the natural gas demanded in billion Btu. *Coal Units* is the proportion of coal plants/operators in the sector. *NG Units* is the proportion of natural gas plants/operators in the sector. *Dual Units* is the proportion of dual-fuel plants/operators in the sector. *Nb. Tot. Gen.* is the overall number of generators by a plant/operator. *Nb. CT. Gen.* is the number of combustion generators. *Nb. ST. Gen.* is the number of steam generators. *Capacity* is the nameplate capacity in Megawatts. *Age/Avg.Age* is the age (average age) of a plant (operator). *Nb. Entities* is the total number of plants/operators in the sector. *Nb. Plants* is the number of plants owned by a operator. *Nb. CHP* is the number of combined heat plants. *Nb. SO2 Cntrl* is the number of plants with a SO2 scrubber. *Nb. NOx Cntrl* is the number of plants with a NOx scrubber. *Nb. Season NOx* is the number of plants with a Particle Matter scrubber. *Nb. ARP* is the number of plants under the Acid Rain Program. *Nb. CAIR NOx* is the number of plants under the CAIR NOx program. *Nb. CAIR SO2* is the number of plants under the CAIR SO2 program. *Nb. Season NOx* is the number of plants under the seasonal NOx program: OTC, NBP, and CAIR OS. *ISO* is the number of plants connected to a ISO/RTO. Regional division is based on the Census region. *Deregulation* is a dummy whether the state has restructured the electricity market. *Capita GDP* is GDP per capita in thousand dollars. *Unemp. Rate* is the unemployment rate. *Retail Electricity Price* is cents per kWh. *Heating Degree Days* is the monthly sum of 65 Fahrenheit minus the average daily temperature. If 65F minus the average daily temperature is negative, HDD is 0 for that day. *Cooling Degree Days* is the monthly sum of the average daily temperature minus 65 Fahrenheit. If the day's average minus 65F is negative, CDD is 0 for that day. *Mean Temperature* is the monthly average of the day's mean temperature. *Relative Humidity* is the monthly mean of the ratio of the partial pressure of water vapor to the saturated vapor pressure of water at a given temperature.

Table 2.9: Summary Statistics on Plant Characteristics

	Plant Level				Operator Level			
	Utility	IPP	Ind/Com	Overall	Utility	IPP	Ind/Com	Overall
Coal (Bn Btu)	3507.1	2459.7	268.5	2524.7	6856.1	3517.8	292.1	3902.4
	(3958.4)	(3464.9)	(475.6)	(3605.3)	(9424.5)	(6128.0)	(512.2)	(7468.1)
NG (Bn Btu)	241.0	470.9	174.3	294.7	443.2	606.7	190.4	412.2
	(878.9)	(937.5)	(1339.8)	(1045.1)	(2128.4)	(1137.2)	(1414.0)	(1614.5)
Coal Units	0.162	0.0876	0.0634	0.115	0.0718	0.0681	0.0608	0.0668
	(0.369)	(0.283)	(0.244)	(0.319)	(0.258)	(0.252)	(0.239)	(0.250)
NG Units	0.677	0.818	0.755	0.739	0.663	0.832	0.754	0.751
	(0.468)	(0.386)	(0.430)	(0.439)	(0.473)	(0.373)	(0.431)	(0.432)
Dual Units	0.161	0.0945	0.181	0.146	0.265	0.0995	0.186	0.182
	(0.367)	(0.293)	(0.385)	(0.353)	(0.441)	(0.299)	(0.389)	(0.386)
Nb. Tot. Gen.	3.502	3.147	2.591	3.167	7.136	4.141	2.838	4.671
	(2.624)	(2.676)	(1.966)	(2.519)	(9.649)	(5.237)	(2.668)	(6.711)
Nb. CT. Gen.	2.156	1.994	1.248	1.879	4.393	2.623	1.367	2.772
	(2.644)	(2.705)	(1.789)	(2.505)	(5.909)	(4.250)	(2.364)	(4.564)
Nb. ST. Gen.	1.345	1.119	1.333	1.274	2.739	1.473	1.460	1.879
	(1.618)	(1.165)	(1.681)	(1.517)	(5.031)	(2.258)	(1.903)	(3.392)
Capacity	406.0	410.5	44.76	316.6	827.2	540.1	49.03	467.0
	(605.1)	(504.1)	(90.86)	(517.0)	(1984.8)	(958.5)	(109.8)	(1301.4)
Age / Avg. Age	34.85	18.53	28.78	28.41	37.73	16.06	28.85	27.41
	(18.71)	(15.82)	(18.73)	(19.19)	(16.82)	(14.24)	(18.57)	(18.85)
Nb. Entities	1370	992	871	1130.8	583	731	758	692.2
	(0)	(0)	(0)	(219.9)	(0)	(0)	(0)	(76.40)
Nb. Plants					2.037	1.316	1.095	1.475
					(2.140)	(1.098)	(0.618)	(1.476)
Nb. CHP					0.00225	0.416	0.972	0.470
					(0.0474)	(0.655)	(0.674)	(0.676)
Nb. SO2 Cntrl					0.230	0.0931	0.0185	0.112
					(0.623)	(0.351)	(0.135)	(0.426)
Nb. NOx Cntrl					0.976	0.832	0.0872	0.627
					(1.758)	(0.978)	(0.286)	(1.227)
Nb. PM Cntrl					0.615	0.215	0.0546	0.290
					(1.357)	(0.634)	(0.227)	(0.897)
Nb. ARP					1.198	0.796	0.0165	0.663
					(2.027)	(1.015)	(0.127)	(1.387)
Nb. CAIR NOx					0.354	0.278	0.00793	0.211
					(1.360)	(0.733)	(0.0887)	(0.897)
Nb. CAIR SO2					0.279	0.218	0.00613	0.166
					(1.211)	(0.649)	(0.0780)	(0.796)
Nb. Season NOx					0.199	0.219	0.0418	0.153
					(1.026)	(0.653)	(0.203)	(0.711)
Nb. ISO/RTO					0.984	0.917	0.684	0.860
					(1.807)	(0.991)	(0.794)	(1.273)
N	349387				236871			

Table 2.10: Summary Statistics on State Characteristics

	East mean/sd	min	max	Midwest mean/sd	min	max	South mean/sd	min	max	West mean/sd	min	max
Deregulation	0.61	0.00	1.00	0.23	0.00	1.00	0.28	0.00	1.00	0.20	0.00	1.00
	(0.49)			(0.42)			(0.45)			(0.40)		
Capita GDP ('000 $)	43.51	23.77	67.66	42.78	30.27	70.77	42.12	24.34	66.13	43.63	25.65	80.00
	(10.48)			(6.15)			(9.26)			(9.72)		
Unemp. Rate	6.18	2.70	12.80	5.63	1.90	14.80	5.99	2.60	12.50	6.23	2.10	14.10
	(2.08)			(2.16)			(2.03)			(2.42)		
Retail Electricity Price	10.79	3.90	20.17	7.25	4.77	12.15	7.93	4.43	14.12	7.53	3.78	15.51
	(3.37)			(1.30)			(1.90)			(2.08)		
Heating Degree Days	14.85	0.00	55.56	17.77	0.00	60.80	8.51	0.00	40.15	15.19	0.00	52.10
	(13.77)			(16.01)			(9.63)			(12.90)		
Cooling Degree Days	2.73	0.00	19.61	2.46	0.00	21.40	5.45	0.00	25.43	2.60	0.00	23.06
	(4.22)			(3.89)			(6.14)			(4.38)		
Mean Temperature	52.89	9.44	84.61	49.69	4.20	86.40	61.95	24.85	90.43	52.00	12.90	86.38
	(16.80)			(18.78)			(14.66)			(15.85)		
Relative Humidity	69.22	50.08	83.05	68.78	40.49	86.29	67.88	42.73	85.22	53.17	14.20	87.56
	(5.49)			(6.86)			(6.14)			(15.60)		

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